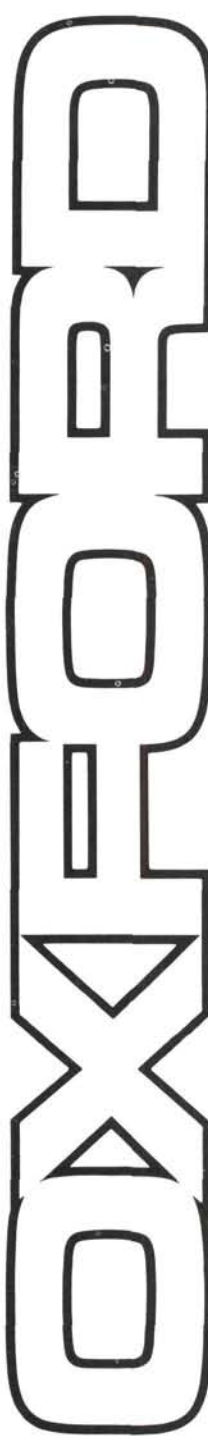


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The Encounter of Two Societies:  
Western Conquerors and Byzantines in the Peloponnesus  
after the Fourth Crusade

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DAVID JACOBY

THE SECOND HALF of the eleventh century witnessed the turning of the tide in the history of the eastern Mediterranean. The Norman conquest of Byzantine Italy and Muslim Sicily and the growing activity of Western merchants in the Byzantine Empire, the Muslim Levant, and Egypt provide two facets of the same phenomenon: together they may be considered as the initial thrust of a rejuvenated West, announcing its imminent military, economic, and demographic expansion eastward. The Crusades were part and parcel of this general phenomenon of Western or Latin expansion, yet they have made a particular imprint in its framework. They brought to Byzantine and Muslim territories the establishment of permanent Latin rule imposed by conquest, resting upon a Latin elite, and reinforced by immigration from the West. The initial phase of conquest is tied to the First Crusade, which enabled the creation of the Kingdom of Jerusalem and the Latin states of Tripoli, Antioch, and Edessa; Byzantine Cyprus was captured by Richard I of England almost by accident in 1191. A second wave of conquest came in the wake of the Fourth Crusade, when Western knights, the Venetian state, and several adventurers acting in their own behalf imposed their rule on Byzantine territories. Finally, Catalans conquered the duchy of Athens in 1311, and Genoese the island of Chios in 1346.

As a result of conquest relations between the Latins and the population of these areas underwent a major change. Commercial activity of Western merchants in Byzantium or Muslim countries in earlier years had called for purely economic and social intercourse with local inhabitants. Temporary or even permanent residence had no bearing on their position as aliens, a status that was further emphasized by their enjoyment of commercial and judicial privileges. But conquest, whether gradual or abrupt,

This is a thoroughly revised version of a paper read in Washington, D.C. on December 28, 1969, at the annual meeting of the American Historical Association. I wish to express my thanks to Professor Bryce Lyon, of Brown University, and Professor Charles M. Brand, of Bryn Mawr College, as well as to other commentators, who have prompted me to clarify or stress certain aspects of the problems I dealt with.

created a problem of a completely different nature: the relationship between an elite of conquerors, their descendants, and the Latins who joined them, on the one hand, and the indigenous population on the other, had to be defined and a pattern of permanent coexistence between the two groups devised. Feudalism transplanted from the West was imposed by a feudal nobility upon areas wrested from the Muslims. In former Byzantine territories the nature of the ruling class of conquerors was more diversified. Norman Italy, Cyprus, and the Peloponnesus witnessed the establishment of feudal rule, while elsewhere a nonfeudal elite wielded authority and held the reins of government. Such was the case in areas subjected to the direct dominion of Venice, especially Crete, as well as in the Catalan duchy of Athens and Genoese Chios. In each instance the very nature of the ruling class as well as the structure of the local society determined to a large extent the character of their encounter, which evidently generated changes in both groups. An investigation of this complex phenomenon in Frankish Morea might well serve as a case study.

ON APRIL 13, 1204, a combined force of Crusaders and Venetians captured Constantinople; the so-called Fourth Crusade had come to an end.<sup>1</sup> The disintegration of the Byzantine Empire, already initiated toward the end of the twelfth century, was accelerated, and within a year large portions of its territory were occupied by Latin conquerors. One of these areas, the Peloponnesus, or the Morea, was invaded in November 1204 by Western knights who gradually extended their rule over the whole peninsula, with the exception of Coron and Modon, which remained under Venetian rule. Although major changes occurred in its boundaries, the Frankish principality of the Morea remained in existence for more than two centuries, eventually disappearing in 1432.<sup>2</sup> The length of Latin rule and the relative wealth of evidence relating to this territory enable us to describe in a fairly accurate way the impact of feudalism on the indigenous population, especially on the Greek upper class, and the changes wrought by this encounter within the Frankish knightly class.

Before dealing with the evolution of society in Frankish Morea, it is,

<sup>1</sup> On the Fourth Crusade, see Edgar H. McNeal and Robert L. Wolff, "The Fourth Crusade," in Kenneth M. Setton, ed., *A History of the Crusades*, 2 (Philadelphia, 1962): 153-85; Antonio Carile, "Partitio Terrarum Imperii Romanie," *Studi Veneziani*, 7 (1965): 125-305; Charles M. Brand, *Byzantium Confronts the West, 1180-1204* (Cambridge, Mass., 1968), 232-69; and Donald E. Queller and S. J. Stratton, "A Century of Controversy on the Fourth Crusade," *Studies in Medieval and Renaissance History*, 6 (1969): 233-77.

<sup>2</sup> The most recent general surveys of Frankish rule in the Morea are by Jean Longnon, *L'empire latin de Constantinople et la principauté de Morée* (Paris, 1949), and by Antoine Bon, *La Morée franque. Recherches historiques, topographiques et archéologiques sur la principauté d'Achaïe (1205-1430)* (Paris, 1969). Unfortunately, many studies on specific subjects published in recent years have not been taken into account by the last author. William of Champlitte assumed the title of prince in 1205. Longnon, *L'empire latin*, 74.



however, imperative to dwell at some length upon the social structure of Byzantium. Despite basic common features, its social structure lacked uniformity: ethnic, economic, and geopolitical factors account for wide diversity within the Empire,<sup>3</sup> and therefore special emphasis should be placed, in our context, on its western provinces. Many important problems relating to the evolution of Byzantine society remain unsolved or open to controversy. This is partly so because evidence is not always available and also because scholars have traditionally focused their attention on fiscal and institutional history. On the other hand, the distinctive features of feudalism as it evolved in the areas of the West from which the Latin conquerors came are better known. Undoubtedly, striking differences existed between Byzantine and feudal societies. Nevertheless, their encounter in Frankish Morea is marked by adaptation, compromise, and integration, a process whose character, stages, and limitations require definition.

TWO OF THE MOST important features of Byzantium, especially when compared with feudal society in the West, are the continuous existence of the state and the nature of Byzantine law. These two closely connected factors exerted a powerful influence on the molding of Byzantine society and made their imprint upon the Byzantine mind. Byzantium inherited from Rome its bureaucratic centralization and an all-embracing administration as well as the basic principles of the political and legal tradition of the Greco-Roman world. These fundamental characteristics were upheld, although time and again the authority of the emperor was challenged and the Empire as a whole submitted to severe strains and stresses. Such was the case shortly before the Fourth Crusade.

The abstract concept of *politeia*, or *res publica*, implied the existence of public law and public taxation. The emperor, who personified the state and was the depository of political and judicial authority, was also the source of all grace and responsible for the implementation of the law.<sup>4</sup> On occasion he granted privileges to individuals, ecclesiastical institutions, or, collectively, to the inhabitants of a city or a territory. These privileges were mostly of a fiscal nature, such as exemption from

<sup>3</sup> See, for example, Speros Vryonis, Jr., "Byzantium: The Social Basis of Decline in the Eleventh Century," *Greek, Roman and Byzantine Studies*, 2 (1959): 161-62.

<sup>4</sup> See Franz Dölger, "Die Kaiserurkunde der Byzantiner als Ausdruck ihrer politischen Anschauungen," *Historische Zeitschrift*, 159 (1938-39): 229-50, reproduced in Dölger, *Byzanz und die europäische Staatenwelt* (Ettal, 1953), 9-33; Louis Bréhier, *Le monde byzantin*, 2, *Les institutions de l'empire byzantin* (Paris, 1949), 1-16, 63-65, 173, 218-19; Herbert Hunger, *Prooimion, Elemente der byzantinischen Kaiseridee in den Arengen der Urkunden* (Vienna, 1964), especially 84-154; Agostino Pertusi, "I principi fondamentali della concezione del potere a Bisanzio," *Bullettino dell'Istituto storico italiano per il medio evo e Archivio Muratoriano*, 80 (1968): 1-23; and Joachim Scharf, "Ius divinum. Aspekte und Perspektiven einer byzantinischen Zweigewaltentheorie," Peter Wirth, ed., *Polychronion, Festschrift Franz Dölger zum 75. Geburtstag* (Heidelberg, 1966), 462-79.

taxes due to the state or the right to collect taxes within the boundaries of an estate and retain them, in part or entirely, instead of passing them on to the imperial fisc. Even so, taxes retained their public character. When access to an estate was forbidden to imperial officers, its possessor or holder was evidently to replace those officers in the exercise of certain functions. Such a grant did not, however, entail the development of private jurisdiction; at best it is comparable to the immunity granted by rulers in the Merovingian and early Carolingian West. In the Empire the concession of administrative, fiscal, or limited judicial powers or exemptions did not amount to a definitive alienation of state prerogatives. The emperor never surrendered his supreme jurisdiction over the inhabitants of the Empire. The limited character both in time and scope of the grants, clearly expressed in the charters delivered, is further illustrated by their renewal at the request of the beneficiaries. The latter were always considered to be imperial agents, empowered by delegation to exert the authority of the state.<sup>5</sup> Nevertheless, when the control of the central government weakened, great landowners in the provinces took hold of the administrative machinery and usurped imperial powers. Such was especially the case in the last years of the twelfth century, shortly before the Frankish conquest.

Among the various grants awarded by the emperors, the *pronoia* has enjoyed particular attention. Literally "provision," it consisted of a concession of state revenues to an individual who collected them directly; to this effect, the emperor transferred to the recipient peasants and the imperial land they cultivated. The *pronoia* originated under the reign of the Comnenoi, toward the end of the eleventh or, more likely, in the early twelfth century, and became more widespread under Manuel I, who ascended the imperial throne in 1143. According to George Ostrogorsky, it was the counterpart of the Western fief; granted on a conditional basis in return for military service, it enabled the recruitment of mounted knights and became the basis of the Byzantine military system. Moreover, it strengthened the landed wealth and power of greater or lesser feudal lords at the expense of the state. The transfer of imperial authority over peasants to the recipients of *pronoiai* had important social

<sup>5</sup> On the nature of the grants, see George Ostrogorsky ("Pour l'histoire de l'immunité à Byzance," *Byzantion*, 28 [1958]: 165-97, 235-46), who has, however, overstressed their judicial aspects. A more balanced view is to be found in Anastasio I. Mouratides, "The Byzantine Immunity System" (Ph.D. dissertation, McGill University, 1965), i-xvi, 80-94, 166-223. See also Nicolas Oikonomides, "The Donation of Castles in the Last Quarter of the 11th Century," *Polychronion*, 413-17; Hélène Glykatzi-Ahrweiler, "La concession des droits incorporels; donations conditionnelles," *Actes du XII<sup>e</sup> Congrès international des Études byzantines* (Belgrade, 1964), 2: 103-14, and "Charisticariat et autres formes d'attribution de fondations pieuses aux X<sup>e</sup>-XI<sup>e</sup> siècles," *Recueil des travaux de l'Institut d'Études byzantines* (Belgrade)-*Zbornik Radova*, 10 (1967): 1-5; see also Paul Lemerle, "Un aspect du rôle des monastères à Byzance: les monastères donnés à des laïcs, les charistikaïres," *Académie des Inscriptions et Belles-Lettres, Comptes-rendus* (Jan.-Mar. 1967), 10.

implications and was a further symptom of the feudalization of Byzantium before 1204.<sup>6</sup> Considering all these factors, it is obvious that the *pronoia* is particularly relevant to our subject—a study of the interaction of Byzantine society with Western feudalism. The question is made the more pressing because twelfth-century sources relating to the *pronoia* are rather scanty, and Ostrogorsky has supplemented them by later evidence drawn from Frankish Morea, in order to clarify both the nature and diffusion of this institution.<sup>7</sup> This procedure casts heavy doubts, however, on the identification of the *pronoia* with the fief in the early thirteenth century.

The later evidence adduced by Ostrogorsky is to be found in the Greek version of the *Chronicle of Morea*, which was composed for a Greek audience in the Peloponnesus in the second half of the fourteenth century, most likely between 1341 and 1388, at least 137 years after the Frankish conquest. This chronicle provides ample information about holders of *pronoiai*, who fought on horseback and were followed by their dependents. *Pronoia* appears in it as identical to fief, and the Greek *archon*, as the counterpart of the Western knight; yet these equations should not be taken at face value. One cannot infer from this late-written source that the meanings of its terms were the same as they had been in the early thirteenth century, nor can one prove thereby the identity of institutions, such as *pronoia* and fief, at the time of the Frankish conquest. This is especially so because the Greek version derives from a French chronicle written for the Frankish feudatories and describing the life of the Moreot princes, barons, and their vassals, as well as their feats of arms, military obligations, mores, way of life, and outlook.<sup>8</sup> The numerous *pronoiai* mentioned in the Greek version are therefore mostly fiefs, and their holders were Frankish feudatories, not members of the Greek upper class.

A careful analysis of the feudal vocabulary used in the Greek version and of several articles of the *Assizes of Romania*, a legal treatise compiled in the Morea between 1333 and 1346, points to two important qual-

<sup>6</sup> See George Ostrogorsky, *Pour l'histoire de la féodalité byzantine* (Brussels, 1954), especially 9–61 for the period under consideration here; see also Ostrogorsky's *History of the Byzantine State* (New Brunswick, 1969), 330–31, 371–72, 392, 425. Answering criticism, the author has recently restated his basic views while slightly correcting them, in "Die Pronoia unter den Komnenen," *Recueil des travaux de l'Institut d'Études byzantines-Zbornik Radova*, 12 (1970): 41–54; see also his "Observations on the Aristocracy in Byzantium," *Dumbarton Oaks Papers*, 25 (1971): 11–12, 14, 17–18.

<sup>7</sup> As emphasized in Ostrogorsky, *Pour l'histoire*, 55, and *History*, 371 n. 2.

<sup>8</sup> On the Greek version, see David Jacoby, "Quelques considérations sur les versions de la *Chronique de Morée*," *Journal des Savants* (July–Sept. 1968), 150–59, 188. An English translation of this version has been published by Harold E. Lurier, *Crusaders as Conquerors: The Chronicle of Morea* (New York, 1964), but see the numerous corrections by Peter Topping, in a review published in *Speculum*, 40 (1965): 737–42. On the French version, see Jacoby, "Quelques considérations," 134–50, 181–87.

ifications.<sup>9</sup> In the first place, the Greek upper class in Frankish Morea did not hold any *pronoia*, in spite of the use of this term by the Greek version to describe their landed estates; second, the term is not used in a legal or technical way and stems anyhow from a late period. The *pronoia* gradually evolved in Byzantine territory into a hereditary tenure, and its military nature became more pronounced under the Palaeologoi, in the second half of the thirteenth century. Because it then resembled the fief the Greeks of the Morea were obviously prompted to use the term *pronoia* to describe the Western institution, and its adoption for this purpose should therefore be placed at that time or perhaps even in the early fourteenth century.<sup>10</sup>

In conclusion, neither backward projection nor generalization on the basis of the Greek version of the *Chronicle of Morea*, as propounded by Ostrogorsky, are warranted. Evidence on the hereditary *pronoia* appearing at a late period cannot be adduced to describe the institution at the time of the Frankish conquest. The same holds true of the portrayal of the mounted knights and their dependents by the Greek version: as Franks are described in this source, the latter provides no clue as to the true nature of the military service performed by the Greek holders of *pronoiai* prior to 1204.<sup>11</sup> Moreover, as the Greek version offers no conclusive evidence about the existence of the *pronoia* in the Peloponnesus at that date, one of the main arguments in favor of its wide diffusion in the whole of the Byzantine Empire has to be dismissed.<sup>12</sup>

Even though only meager evidence remains after the Greek version of the *Chronicle of Morea* has been rejected, it still appears that the *pronoia* of the twelfth century was basically different from the Western fief in several respects. It was granted only for the lifetime of the recipient and could be neither alienated nor enfeoffed, subinfeudation being non-existent in Byzantium. *Pronoiai* were not always conceded in return for military service, and there is no indication whatsoever that their holders constituted either an important factor in the Byzantine army or a military class.<sup>13</sup> Finally, it should be stressed that the concession of a

<sup>9</sup> For the dating of the *Assizes of Romania*, see my book *La féodalité en Grèce médiévale. Les "Assises de Romanie": sources, application et diffusion* (Paris, 1971), 75–82. The text of this treatise has been edited by Georges Recoura, *Les Assises de Romanie* (Bibliothèque de l'École des Hautes Etudes, fasc. 258) (Paris, 1930) (hereafter *Assises*). An English translation has been provided by Peter W. Topping, *Feudal Institutions as revealed in the Assizes of Romania, the Law Code of Frankish Greece* (Philadelphia, 1949), 15–99.

<sup>10</sup> Criticism of Ostrogorsky is to be found in my study "Les archontes grecs et la féodalité en Morée franque," *Travaux et Mémoires du Centre de recherche d'histoire et civilisation byzantines*, 2 (1967): 421–65. Ostrogorsky has obviously misread my article. See "Die Pronoia," 52–53. I have never claimed that the *pronoia* as an institution had been introduced in the Peloponnesus when it was under Frankish rule, but that such was the case with the term used to describe it.

<sup>11</sup> Contrary to the assertion of Ostrogorsky, *History*, 371 n. 2.

<sup>12</sup> In favor of a wide diffusion, see Ostrogorsky, *Pour l'histoire*, 55; cf. Jacoby, "Les archontes," 479–81, to the contrary.

<sup>13</sup> Contrary to Ostrogorsky ("Die Pronoia," 42–43), who has recently emphasized again the

*pronoia* did not entail a grant of public authority or jurisdiction over the peasants tilling its land.<sup>14</sup> The nature of political power and the distribution of authority in Byzantium before 1204 do not warrant the assumption that a process of feudalization was under way in the Empire, nor did the *pronoia* necessarily play a significant part in undermining its strength.<sup>15</sup>

Considerations about the social structure of Byzantium provide additional evidence that fundamental differences existed between twelfth-century Byzantium and feudal society in the native countries of the conquerors. Byzantine law and society inherited the clear-cut distinction between slaves and free men. Legally, all free men, whether relatives of the emperor, imperial officials, members of the Senate, or "commoners," were the *douloi*, or servants, of the ruler<sup>16</sup> and were governed by the same law system regardless of their social status or their relationship to him. On the whole, it was the aim of the emperor to ensure that all his subjects should enjoy the same treatment in law courts,<sup>17</sup> but in practice, judicial discrimination could not always be prevented: judges were often impressed by the social standing of the parties appearing before them, and the exertion of social and economic pressure or the use of bribery were evils that beset Byzantium as they did other medieval societies.<sup>18</sup> Still, as the nature of imperial grants shows, preferential treatment in law courts was not based on a legal concept of differentiation. Whether conceded to landlords, dignitaries, officials, or soldiers, titles and priv-

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exclusively military character of this institution, and to "Observations," 11 and 14, where the author implies the existence of contingents of holders of *pronoia*, for which no evidence can be adduced. I shall return elsewhere to this subject. On the composition of the Byzantine army in the late eleventh and twelfth centuries, see mainly Paul Lemerle, "Recherches sur le régime agraire à Byzance: la terre militaire à l'époque des Comnènes," *Cahiers de civilisation médiévale*, 2 (1959): 265-81; see also Hélène Ahrweiler (*Byzance et la mer* [Paris, 1966], 205-22, 397-407), who relies, however, too heavily on Ostrogorsky in relation to the diffusion of the *pronoia*.

<sup>14</sup> See above, n. 5. The presumed analogy of the *pronoia* with the Western fief has already been discarded by Miloš Mladenović, in "Zur Frage der Pronoia und des Feudalismus im byzantinischen Reiche," *Südost-Forschungen*, 15 (1956): 123-35, 137-39.

<sup>15</sup> Ostrogorsky ("Observations," 8-9) waters down the concept of feudalism by stating that its expression is "the presence of the seignorial estate peopled by dependent peasants," which might apply to many countries and societies, even in antiquity. A penetrating analysis is offered by Claude Cahen ("Réflexions sur l'usage du mot de 'féodalité'. A propos d'un livre récent," *Journal of the Economic and Social History of the Orient*, 3 [1960]: 4-20), who emphasizes the importance of the exercise of public authority and prerogatives by private persons as an essential ingredient of feudalism.

<sup>16</sup> Jean Verpeaux, "Les oikcioi. Notes d'histoire institutionnelle et sociale," *Revue des études byzantines*, 23 (1965): 96-97.

<sup>17</sup> See Bréhier, *Le monde byzantin*, 2: 173, 219. The same principle was restated at a later date, in 1334, by Emperor Andronicus III. Paul Lemerle, "Le juge général des Grecs et la réforme judiciaire d'Andronic III," *Mémorial Louis Petit, Mélanges d'archéologie et d'histoire byzantines* (Bucharest, 1948), 299-300.

<sup>18</sup> In case of abuse one could seek redress from the emperor. A suggestive study on the discrimination against the poor in contemporary American society is provided by Jerome E. Carlin, Jan Howard, and Sheldon L. Messinger, "Civil Justice and the Poor: Issues for Sociological Research," *Law and Society Review*, 1 (1966): 9-90, also published separately (New York, 1967).

ileges were always bestowed on an individual basis and never collectively upon a specific group enjoying the same social standing.<sup>19</sup> They provided no exemption from regular judicial procedure for individuals, nor did they give rise to a class endowed with a particular legal status or governed by a particular set of laws.

Legal equality in Byzantium should not be mistaken for social or economic equality, nor did it necessarily afford equal chances of promotion in society.<sup>20</sup> High-ranking birth was often emphasized; the "well born" or children of "well-born parents" were favored at the outset<sup>21</sup> and upstarts utterly despised.<sup>22</sup> Definitions of class boundaries remained vague, however. Terms such as "powerful" (*dynatoi*) or "poor" (*aporoí*, *ptochoi*) implied mainly a social and, additionally, an economic standing, not necessarily identical.<sup>23</sup> Yet high-ranking birth or differences in social status, economic status, power, or prestige never assumed a legal character and were never sanctioned or perpetuated by law.<sup>24</sup> This fundamental characteristic of Byzantine society stands out in sharp contrast to feudal society as it evolved in the native countries of the conquerors: in the feudal West of the late twelfth century, social classes were synonymous with legal classes, and status had become hereditary. To be sure, aristocracy existed in Byzantium as a social class, and certain families belonging to this group played a dominant role in the evolution of the Empire,<sup>25</sup> but no hereditary nobility enjoying a particular legal status is to be found.<sup>26</sup> Moreover, the absence of clearly defined legal classes<sup>27</sup> or a rigid system of stratification enabled a constant trend of upward mobility and inclusion within the elite of the Empire. To some extent Byzantine society

<sup>19</sup> The individual character of the privileges should be stressed in our context. See Jacoby, "Les archontes," 466-67, especially nn. 231, 236.

<sup>20</sup> This is especially the case for the *paroikoi*, who by the late twelfth century formed the major part of the Byzantine peasantry; although legally free, they were subjected to severe personal restrictions. I shall deal elsewhere with this subject.

<sup>21</sup> See Romilly J. H. Jenkins, "Social Life in the Byzantine Empire," *Cambridge Medieval History*, vol. 4, pt. 2, ed. Joan M. Hussey (Cambridge, 1967), 99-100, and below, n. 25; see also Ostrogorsky, "Observations," 4-5.

<sup>22</sup> See, for example, Brand, *Byzantium Confronts the West*, 143.

<sup>23</sup> See especially Paul Lemerle, "Esquisse pour une histoire agraire de Byzance: les sources et les problèmes," *Revue historique*, 219 (1958): 268-80.

<sup>24</sup> Evidence to this effect is found in Monemvasia, in the southern Peloponnese. See Jacoby, "Les archontes," 466 n. 231.

<sup>25</sup> See, for example, Vryonis, "Byzantium," 161-64; see also Nicolas Svoronos, "Société et organisation intérieure dans l'empire byzantin au XI<sup>e</sup> siècle: les principaux problèmes," *Thirteenth International Congress of Byzantine Studies* (Oxford, 1966), Main Papers, 12: 1-7.

<sup>26</sup> See Rodolphe Guiland, "La noblesse de race à Byzance," *Byzantinoslavica*, 9 (1947-48): 307-14; Guiland, "La transmission héréditaire des titres nobiliaires à Byzance," *Kodai gaku (Palaeologia)*, 8 (1959): 137-43; and Guiland, "La noblesse byzantine. Remarques," *Revue des études byzantines*, 24 (1966): 40-42, 49-52. The first two studies have been reprinted in Rodolphe Guiland, *Recherches sur les institutions byzantines*, 1 (Berlin, 1967), respectively 15-22 and 65-72. See also the important remarks by Franz Dölger, in his review of Philip P. Argenti, *Libro d'Oro de la noblesse de Chio* (Oxford, 1955), published in *Byzantinische Zeitschrift*, 49 (1956): 126-27.

<sup>27</sup> With the exception of slaves and *paroikoi*. See above, n. 20.



was an "open" society and remained so toward the end of the twelfth century. One can find many instances in which *homines novi*, even of lowly origin, gained access to the upper class;<sup>28</sup> several of them were to ascend the imperial throne. Inclusion within the elite of the Empire was not necessarily attained in the wake of dynastic changes. Education and intellectual achievement were highly valued in Byzantium;<sup>29</sup> so were administrative efficiency and military valor. The imperial bureaucracy, the army, and serving powerful men<sup>30</sup> provided avenues for social promotion and consequently afforded chances to acquire land and power, sometimes on a large scale.<sup>31</sup> The progressive development of a class of rich townspeople, beginning toward the end of the tenth century, added a new factor of mobility. From about the middle of the eleventh century, emperors appointed several of them to the Senate and to high imperial offices.<sup>32</sup>

Powerful men, dignitaries, and great landowners occasionally had armed retainers; great landowners also exerted some authority over the free peasants tilling their lands, especially when these landowners were vested with certain fiscal or administrative powers granted to them by the emperor. Personal bonds of dependence, however, always retained their private nature and were never recognized by law or sanctioned by custom.<sup>33</sup> Vassalage as it was known in the West was alien to the Byzantine political structure and to Byzantine thought. Obviously, contact with Latins, even before the First Crusade, had acquainted Byzantium with feudal institutions, although the encounter did not prepare the ground for their acceptance in the Empire or change the relationship of the emperor with his subjects. Significantly, vassalage appeared in Byzantium only when the emperor established such a relationship with Latins, which

<sup>28</sup> Such as Philokales, formerly a peasant, mentioned in a novel of Emperor Basil II dated 996. See Lemerle, "Esquisse," 277-78. An English translation of this text is provided by Charles M. Brand, *Icon and Minaret: Sources on Byzantine and Islamic Civilization* (Englewood Cliffs, N. J., 1969), 91-97, and see especially 93. For the twelfth century, see the example mentioned in Brand, *Byzantium Confronts the West*, 143. Many eunuchs were of lowly origin; on their importance, see Rodolphe Guiland, "Les eunuques dans l'empire byzantin," in *Recherches*, 1: 165-97. See also below, n. 30. Ostrogorsky ("Observations," 4-5, 9, 29) insists upon the existence of a hereditary aristocracy, but the evidence adduced throughout his study is not convincing and, besides, points to upward mobility into the upper class. A thorough study of this phenomenon is still wanting.

<sup>29</sup> On which see Jenkins, "Social Life," 80; Ostrogorsky, "Observations," 29-30; and Paul Lemerle, *Le premier humanisme byzantin* (Paris, 1971), 255-56.

<sup>30</sup> See especially Hans-Georg Beck, "Byzantinisches Gefolgschaftswesen," in *Bayerische Akademie der Wissenschaften, Philosophisch-historische Klasse, Sitzungsberichte, Jahrgang 1965* (Munich, 1965), 5: 1-31; Verpeaux ("Les oikeioi," 89-99), who also deals with the period following the Fourth Crusade; and Ostrogorsky, "Observations," 12-16.

<sup>31</sup> The successful career of an intellectual of lowly origin is described by Nicoletta Dujé, "Un haut fonctionnaire byzantin du XI<sup>e</sup> siècle: Basile Malèsès," *Revue des études byzantines*, 30 (1972): 167-78.

<sup>32</sup> See Svoronos, "Société et organisation intérieure," 8-10.

<sup>33</sup> See above, n. 30. Ostrogorsky ("Observations," 14-15) agrees to this fact, but nevertheless arrives at the surprising conclusion that dependents occasionally were not directly subject to the emperor.

explains the exclusive use for this purpose of Western feudal terminology, transliterated or translated into Greek.<sup>34</sup>

Great landowners played a dominant role in the provinces of the Empire. Their estates—a source of wealth, prestige, and power—assured them of leadership on a regional or local scale and of support by their dependents, whether these were inhabitants of small cities or small landholders.<sup>35</sup> Yet many great landowners strove to enhance their social status and strengthen their influence by acquiring administrative or military functions within the imperial machinery of government. Honorary titles, which assured a standing in the imperial hierarchy, were eagerly sought and, if not granted, bought from the emperor.<sup>36</sup> It is therefore not surprising that the same word, *archon*, applied to great provincial landowners as well as to imperial officials, the latter vested with administrative or military authority. One occasionally would make a distinction between the rich landlord, or *ktematikos archon*, and the official in charge of civilian administration or the army officer, known respectively as *thematikos* and *tagmatikos archon*.<sup>37</sup> Although many *archontes* derived their social status from rural estates, those of the Western provinces of Byzantium seem on the whole to have resided in cities.<sup>38</sup> As some of these cities served as administrative and military centers, it is not surprising that they afforded a powerful asset when *archontes* took advantage of the weakness and eventual collapse of imperial authority on the eve of the Latin conquest. Several such cases in the Peloponnesus are also illustrative of the dual position of many *archontes* as great landowners and imperial officials, a position fully exploited to foster political ambitions.

Among these *archontes* Leo Sgouros achieved the most spectacular career. In Nauplia he inherited the powerful standing of his father, who had been the virtual ruler of the city. He appears to have been an imperial officer in 1198 and seems to have borne an official title in 1203.

<sup>34</sup> See Jacoby, "Les archontes," 440, 476, and Verpeaux, "Les oikeioi," 93-94. Bohemund of Antioch uses Western terminology to express the relationship of the princes of Lesser Armenia to Emperor Alexius I. Anne Comnène, *Alexiade*, ed. Bernard Leib (Paris, 1945), 3: 134 (ll. 1-4).

<sup>35</sup> For the eastern provinces, see above, n. 25. In the Peloponnesus this is apparent in Monemvasia and in the case of Sgouros, on whom see below.

<sup>36</sup> For the tenth to twelfth centuries, see Paul Lemerle, "Roga et rente d'état aux X<sup>e</sup>-XI<sup>e</sup> siècles," *Revue des études byzantines*, 25 (1967): 77-100; Armin Hohlweg, *Beiträge zur Verwaltungsgeschichte des oströmischen Reiches unter den Komnenen* (Munich, 1965), 34-40.

<sup>37</sup> See Jacoby, "Les archontes," 465-66, and below, on Sgouros and Chamaretos; see also Hélène Glykatzi-Ahrweiler, "Recherches sur l'administration de l'empire byzantin aux IX<sup>e</sup>-XI<sup>e</sup> siècles," *Bulletin de correspondance hellénique*, 84 (1960): 83 n. 4.

<sup>38</sup> Nicolas G. Svoronos, "Recherches sur le cadastre byzantin et la fiscalité aux XI<sup>e</sup> et XII<sup>e</sup> siècles: le cadastre de Thèbes," *Bulletin de correspondance hellénique*, 83 (1959): 67-77, 141-44; Henri de Valenciennes, *Histoire de l'empereur Henri de Constantinople*, ed. Jean Longnon (Paris, 1948), par. 672, mentions the welcome of *papas*, *alcontes*, and other inhabitants of Thebes to the Latin emperor Henry in 1209. On the *kastrenoi* of Athens toward the end of the twelfth century and the *archontes* of the Peloponnesus, see Jacoby, "Les archontes," 427-28, 466-67, and below.

From Nauplia he extended his power in 1202 by seizing Argos and Corinth and allied himself with pirates of Aegina and Salamis in order to subdue Athens, but to no effect. He successfully assaulted Thebes and advanced in Thessaly in 1204–05, his power finally collapsing when he faced the Frankish invaders. Another *archon*, Leo Chamaretos, who bore the official title of *proedros* of Lacedaemonia, in 1205 managed briefly to impose his authority on the area surrounding Sparta.<sup>39</sup> At about the same time an *archon* of the region of Modon, in the southwestern Peloponnesus—his identity and exact standing cannot be ascertained—<sup>40</sup> allied himself with Geoffrey of Villehardouin, one of the leaders of the Franks, hoping to conquer the peninsula with him. The position of the Slav *archontes* was somewhat different. Several groups of Slav population who settled in Byzantine territory from the late sixth to the early ninth century preserved their particular features and their tribal structure—and this was especially the case in the Peloponnesus. The imperial government included them within the framework of its military organization and, in return for military service, granted them fiscal privileges. Moreover, the emperor recognized the authority and strengthened the traditional status of their chiefs by conferring on them imperial titles, and it is therefore not surprising that they were considered *archontes*.<sup>41</sup>

As leaders of the local population at the time of the Frankish conquest, the *archontes* of the Peloponnesus headed the struggle against the invaders. Some *archontes* perished, others fled;<sup>42</sup> most of them came to terms with the Latin invaders or were eventually compelled to accept their rule. A major change was to take place in their status.

THE FRANKISH ARMY that conquered the Peloponnesus included knights from widely scattered areas in the West, extending from Flanders to Provence, though most of them came from Champagne and Burgundy.<sup>43</sup> Despite differences in their backgrounds they brought with them institutions, traditions, attitudes, and values common to feudal society in the

<sup>39</sup> See Jacoby, "Les archontes," 466–67; Brand, *Byzantium Confronts the West*, 152–54, 244–45; Antoine Bon, *Le Péloponnèse byzantin jusqu'en 1204* (Paris, 1951), 123, 172–73, 204–05, nos. 67 and 68.

<sup>40</sup> Geoffroy de Villehardouin, *La conquête de Constantinople*, ed. Edmond Faral (Paris, 1938–39), 2: 134–36, pars. 325–26. Karl Hopf (*Geschichte Griechenlands vom Beginn des Mittelalters bis auf unsere Zeit*, *Ersch-Gruber Encyklopädie*, 85 [Leipzig, 1867–68], 212 b) identified him with a member of the Kantakouzenos family, but this is without foundation. See Donald M. Nicol, *The Byzantine Family of Kantakouzenos (Cantacuzenus), ca. 1100–1460, A Genealogical and Prosopographical Study* (Dumbarton Oaks Studies, 11) (Washington, 1968), 7 n. 15.

<sup>41</sup> This is obvious in the case of the Melings living in Laconia. See Jacoby, "Les archontes," 465–66, also 443–44; in addition see Hélène Ahrweiler, "Le sébaste, chef de groupes ethniques," *Polychronion*, 34–38, especially 36–37.

<sup>42</sup> Such a case is mentioned in a letter by Michael Choniates. Spyridon Lampros, *Michael Akominatou tou Choniatou ta sozomena* (Athens, 1879–80), 2: 276–80 (in Greek).

<sup>43</sup> Jean Longnon, "Problèmes de l'histoire de la principauté de Morée," *Journal des Savants* (Apr.–June 1946), 86–87.

northern part of the Capetian kingdom and neighboring areas, such as the county of Burgundy, toward the end of the twelfth century.<sup>44</sup>

Feudal society in these territories was by then strictly stratified, basic social status being synonymous with legal status and, most important, transmitted by heredity. Each class was governed by a particular legal system. Since the eleventh century there were, to be sure, constant streams of mobility within the various social classes, which lacked both social and economic homogeneity. But, whether vertical or horizontal, this mobility had no bearing on the legal status of an individual. Non-nobles serving on a permanent basis in the administration or as horsemen in the armies of kings, dukes, counts, and barons thereby could enhance their social and economic position and enjoy special prestige, although they were prevented by a legal barrier from joining the ranks of the nobility. Neither did the holding of a feudal tenement by *ministeriales* or serjeants, attested to in the twelfth century, entail any change in their personal status.<sup>45</sup> Social promotion involving the crossing of class boundaries was by then essentially restricted to the lower strata of society.<sup>46</sup> Servile peasants became free by moving to new lands or by settling in towns; townspeople acquired personal freedom in different ways. The change in their social and legal status implied a legal procedure or some formal act to this effect, such as the grant of an individual, or collective, charter by a feudal lord or, in the case of a peasant who had settled in a town, testimony as to the length of his stay there. Legal procedure of this nature was especially imperative in order to gain promotion to the upper class of feudal society. The development of class-consciousness within the ranks of the feudatories, illustrated by the ceremony of dubbing to knighthood, made access to the latter status most difficult, although not impossible. By the late eleventh century nobility was already considered a matter of blood, and by 1200 chivalry had become an order, with its specific ritual, morals, and obligations.<sup>47</sup> Nevertheless, one could still acquire noble status by being knighted. It is true

<sup>44</sup> On this matter and on the discussion that follows, see especially Marc Bloch, *La société féodale* (Paris, 1939-40), whose views have been corrected on several points; Georges Duby, *La société aux XI<sup>e</sup> et XII<sup>e</sup> siècles dans la région mâconnaise* (Paris, 1953), and, by the same author, "Une enquête à poursuivre: la noblesse dans la France médiévale," *Revue historique*, 226 (1961): 1-22; Jean Richard, *Les ducs de Bourgogne et la formation du duché du XI<sup>e</sup> au XIV<sup>e</sup> siècle* (Dijon, 1954); and Robert Boutruche, *Seigneurie et féodalité*, 2: *L'Apogée (XI<sup>e</sup>-XIII<sup>e</sup> siècles)* (Paris, 1970). The county of Burgundy was included in the Empire, but family ties and constant contacts account for an evolution of feudalism close to that of the duchy of Burgundy. See Richard, *Les ducs de Bourgogne*, and Jean-Yves Mariotte, *Le comté de Bourgogne sous les Hohenstaufen, 1156-1208* (Paris, 1963).

<sup>45</sup> See Duby, *La société*, 241, 381-96, 436-37; Boutruche, *Seigneurie et féodalité*, 2: 285-87.

<sup>46</sup> On the other hand, downward mobility is frequently attested for members of the nobility. See Duby, *La société*, 241-43, 411-18, and Edouard Perroy, "Social Mobility among the French Noblesse in the Later Middle Ages," *Past and Present*, no. 21 (Apr. 1962): 25-38.

<sup>47</sup> See the full discussion of the problem by Duby, *La société*, 230-45, 411-18, 630-33; see also Duby, "Les origines de la chevalerie," in *Ordinamenti militari in Occidente nell'alto medioevo* (Settimane di studio del Centro italiano di studi sull' Alto Medioevo, XV, 2) (Spoleto, 1968), 739-61.

that the privilege of granting knighthood tended to become restricted to a few and that the number of beneficiaries remained on the whole small. But this restriction belongs to the first half of the thirteenth century and grows out of the sharp economic rise of certain burgesses as well as the constant progress of royal power in Capetian France, both of which had been initiated in the preceding century.<sup>48</sup>

The impersonal concept of state remained alien to the mind of feudal society. Personal bonds, of a private nature, provided the backbone of social and political hierarchy; they were sanctioned by custom, governed by strict rules, and had legal force. The organization of society was reflected in institutions and practices such as homage, fealty, and fief. Knights formed the dominant class of society, but wide differences in standing, power, and wealth can be perceived within their ranks. Judicial and legislative authority as well as the right of taxation were vested in private hands and exercised by the upper ranks of feudal lords, although, as a result of centralization, the kings of France and the various counts and dukes also exercised public authority in these matters. Most knights lived on their own rural estates or in the court of a rural lord. Warriors by vocation, they strictly refrained from any direct involvement in economic activity, whether in agriculture or in commerce. Although this attitude was an integral part of their social ethos, it did not prevent them from occasionally pursuing a conscious economic policy implemented by their servants or subjects. In the twelfth century knights in the areas from which the conquerors came normally enjoyed exemption from regular taxes or payments of a rent in money; commutation of military service, relief, aids, and general levies were not, of course, considered as such.<sup>49</sup> The very style of life of the knights reflected their values and mentality. Feudal vocabulary itself faithfully expressed their sense of social superiority and their attitude toward the inferior classes. As the occasional performance of homage by non-nobles suggests, the ethos and concepts of the nobility pervaded the whole of society.<sup>50</sup>

IT COULD HARDLY be expected that feudalism, with its particular social structure, political organization, and institutions, would be transplanted from the West to the Peloponnesus without undergoing any changes. While retaining its essential characteristics, it had to adapt to new cir-

<sup>48</sup> See Duby, *La société*, 522-23, 579-81, 623, 629, 634-36, and Boutruche, *Seigneurie et féodalité*, 2: 284-89; see also Perrov, "Social Mobility," 25-38, but the evidence adduced from the Forez does not necessarily warrant generalization.

<sup>49</sup> Some small fiefs were, however, liable to periodic payments and services instead of military service, but this was rather exceptional. See Boutruche, *Seigneurie et féodalité*, 2: 202-03.

<sup>50</sup> On this last point, see Georges Duby, "La féodalité? Une mentalité médiévale," *Annales: Économies, Sociétés, Civilisations*, 13 (1958): 765-71; Duby, "The Diffusion of Cultural Patterns in Feudal Society," *Past and Present*, no. 39 (Apr. 1968), 3-10; see also Kenneth-John Hollyman, *Le développement du vocabulaire féodal en France pendant le haut moyen âge* (Geneva, 1957); Boutruche, *Seigneurie et féodalité*, 2: 164-65, 170-72, 287; and Jacoby, "Les archontes," 463.

cumstances, to make compromises and adjustments. Gradations of power and rank within the conquering host were inevitably reflected in Morea. In addition the Franks had to take into account a virtually permanent state of war, first during the period of conquest, and again, from 1262 on, when Byzantium sought to expand her rule in the Peloponnesus after regaining a foothold in the peninsula.<sup>51</sup> Finally, the existing structure of Byzantine society was to exert an influence on the class of Frankish knights.

In their native countries the knights who conquered the Morea had with few exceptions belonged to the intermediate and lower ranks of the nobility. One discovers among them a small group of leaders adorned with special prestige. They in turn were followed by knights and serjeants, many of whom had already been their vassals and dependents in the West, while others had joined them in the course of the Crusade or the conquest of the Peloponnesus. The initial gap between leaders and vassals persisted and even widened when the conquerors settled in the Morea. For lack of land only a small number of baronies was created, most of them consisting of four to eight fiefs.<sup>52</sup> Enfeoffment of knights and mounted serjeants was necessarily restricted by the prince and the barons, a relatively large demesne being essential for the preservation of their political, social, and economic ascendancy. Grants of land did not exceed one-third of their fiefs,<sup>53</sup> a rule imposed on petty feudatories to ensure the performance of the military service to which they were liable.<sup>54</sup> It is therefore not surprising that many knights held only one fief or part of a fief and mounted serjeants half a fief or even less;<sup>55</sup> grants were occasionally restricted to the lifetime of the recipient

<sup>51</sup> This situation accounts for many of the rules governing the fief and military service in the Morea. See Jacoby, "Les archontes," 434-35, 447-51, 459-60, and "La féodalité," 54-55; also below, about the *archontes*. Yet relations between Byzantium and Frankish Morea were peaceful for long periods. Jacoby, "Un régime de coseigneurie gréco-franque en Morée: les 'casaux de parçon,'" *Mélanges d'Archéologie et d'Histoire publiés par l'École Française de Rome*, 75 (1963): 111-25.

<sup>52</sup> For all this, see my study "La Morée franque. Bilan et perspectives de recherches," to appear in *Le Moyen Âge*.

<sup>53</sup> As illustrated by the grant by Prince William II of eight fiefs, out of twenty-four belonging to the barony of Akova, to Marguerite of Passavant. See the French version of the *Chronicle of Morea*, ed. Jean Longnon, *Livre de la conquête de la principauté de l'Amorée, Chronique de Morée (1204-1305)* (Paris, 1911) (hereafter *French chronicle*), pars. 525-27, 531.

<sup>54</sup> *Assises* (see above, n. 9, in fine), arts. 30, 46, 72; see also art. 107 and Jacoby, "Les archontes," 450.

<sup>55</sup> After enumerating the baronies, the Greek version of the *Chronicle of Morea* states: "The knights who had one fief each and also the serjeants who were enfeoffed I do not name because of the amount of writing it would require." *The Chronicle of Morea, To chronikon tou Moreos*, ed. John Schmitt (London, 1904) (hereafter *Greek chronicle*), v. 1965-67. For parts of fiefs, see above and previous note; also *Assises*, arts. 57, 89, and the examples provided below. Mounted serjeants as a rule held a serjeanty, a feudal tenement providing an income worth half that of a fief liable to a knight's service. Jacoby, "Les archontes," 449. Serjeants enfeoffed with part of a serjeanty are mentioned in the Aragonese version of the *Chronicle of Morea*, ed. Alfred Morel-Fatio, *Libro de los fechos et conquistas del principado de la Morea* (Geneva, 1885) (hereafter *Aragonese chronicle*), pars. 133, 139.



or of the donor.<sup>56</sup> The existence of money fiefs and of household knights further emphasizes the strong position of the prince and the barons on the one hand and on the other, the precarious standing of most members of the knightly class and of the serjeants.<sup>57</sup>

All these factors account for the particular structure of the feudal hierarchy in Frankish Morea. It had only three ranks below the prince, who headed the pyramid. Next to him came his direct vassals, whether lieges or feudatories of simple homage; among the lieges the barons of the principality enjoyed a special position as his tenants-in-chief and were referred to as "peers of the prince."<sup>58</sup> In turn, all the lieges of this first rank could have vassals of their own, and such was also the case with lieges of the latter rank.<sup>59</sup> The rite of homage performed was directly related to social and legal status: all noble feudatories did liege homage whereas non-noble serjeants did plain homage, a difference which accounts for the fact that the latter could have no vassals of their own.<sup>60</sup> Differences in rights and prerogatives between barons, other lieges, and feudatories of simple homage are illustrated in the field of jurisdiction.<sup>61</sup> Like the prince, barons had rights of low and high justice, the latter including criminal jurisdiction, whereas the other lieges enjoyed only the exercise of low justice.<sup>62</sup> As for the feudatories of simple homage, they did not participate in formal court gatherings convened by their lords, had no court of their own, and their judicial competence was restricted to civil cases of their dependent peasants, the *villani*, or vil-

<sup>56</sup> *Assises*, art. 98; Jacoby, "Les archontes," 451, 459.

<sup>57</sup> On money fiefs, see Jacoby, *La féodalité*, 135-36, 186, 224; household knights are mentioned below.

<sup>58</sup> On the barons as peers of the prince, see *Assises*, art. 43 (first part); as lieges of the prince, see *Assises*, arts. 2, 48, where the translation should read: "the liegemen (i.e., laymen) and prelates who have a barony"; see also arts. 22, 49. The most recent treatment of liege homage is by Boutruche, *Seigneurie et féodalité*, 2: 162-70.

<sup>59</sup> *Assises*, art. 12, on feudatories of a baron or a "chavalier"; this last term is synonymous with liege, as illustrated by the enumerations in art. 14 ("baron, cavalier o feudatario"), art. 2 ("baroni et homeni ligii . . . et li altri afeudadi"), and arts. 49 ("baroni et altri cavalieri e feudatarii ligii"), 207, 216, barons and lieges have their own lieges; see also art. 28, where the first "feudatario" (l. 2) is a liege, and art. 216. In art. 94 "baron" is synonymous with liege of the prince.

<sup>60</sup> "Que todos los varones et los cavalleros et los nobles escuderos fiziessen omenage de ligious, et los otros escuderos et nobles Griegos fizieron omenage de plano." *Aragonese chronicle*, par. 137. The noble squires mentioned in the first category were of noble descent, but had not yet been dubbed, as we shall also see below. The non-noble squires were serjeants, to whom the "noble Greeks" were assimilated. This process also will be examined below. The rite of liege homage is described in *Assises*, arts. 3, 68. The feudatories of simple homage had no court of their own and therefore no vassals. Lieges were entitled to hold additional fiefs by simple homage (*Assises*, arts. 34, 68) or several fiefs from different liege lords (arts. 46, 63, 99).

<sup>61</sup> See the classification in *Assises*, art. 1 (where *fedeli* is identical to *afeudadi* in art. 2); see also arts. 2 (cited above, n. 59), and 143 ("de la corte de algun baron, de legio o de feudatario"). On judicial prerogatives, see *Assises*, arts. 42, 43, 48, 49, 72, 94, 143, 151, 162, 186, 203, 207.

<sup>62</sup> In the feudal West high justice included criminal cases involving the death penalty or mutilation as well as important civil cases relating to the legal status of persons and property. Low justice was exercised in criminal as well as civil cases of minor importance, concerning rents and dues for land, for example.

lains.<sup>63</sup> It is apparent, then, that social differentiation within the Frankish elite was most pronounced. The power of the barons, their extensive prerogatives, which included the right to erect castles,<sup>64</sup> their style of life, in the midst of their vassals and dependents, all these singled them out as an exclusive group.<sup>65</sup> The cleavage between the vassals of simple homage, the lowest stratum of the feudal hierarchy, and other feudatories was no less marked. Although feudatories, they were not members of the knightly class, a fact that proved to be of particular importance to the evolution of Moreot society and that accounts partly for the attitude adopted by the Franks toward the Greek upper class.<sup>66</sup>

Despite vast differences within their ranks, the Frankish knights displayed common values and attitudes, which were shared by the Western knights who joined them in the Morea in the second half of the thirteenth and in the fourteenth centuries.<sup>67</sup> They were imbued with a powerful class-consciousness that had matured in the native countries of the conquerors and was further enhanced by the successful conquest and conditions prevailing in the Morea. Their numerical inferiority in the midst of a large Greek population was especially obvious in isolated castles and rural mansions; nevertheless, they manifested no fear of being submerged. Confidence in their own survival as a ruling class was inspired by a genuine sense of social and military superiority, most pronounced in the baronial group. This sense of superiority is reflected in the matrimonial policy pursued by the knights: many of them married daughters of noble families in France and subsequently brought their wives to the Morea; others wedded Latin women from various areas of the West or the Morea, while intermarriage with Greeks is seldom attested.<sup>68</sup> Their relative seclusion and particular style of life afforded additional guarantees of social continuity. Most of them lived in the repaired or enlarged acropolis of a city, an isolated mountain-castle or a fortified rural mansion, apart from Greek society and its economic activity, but this did not prevent them from residing occasionally in the houses they held in cities.<sup>69</sup> Festive

<sup>63</sup> See especially *Assises*, arts. 72, 42. At the end of art. 72 it is stated that "a liege man has a court while a man of simple homage cannot have one." See also Jacoby, "Les archontes," 468-69.

<sup>64</sup> *Assises*, art. 94.

<sup>65</sup> In 1304 the powerful Nicholas III of Saint-Omer insisted that Vincent de Marais, vassal and counselor of Prince Philip of Savoy, should leave the princely court gathering, for "he was not worthy nor a man [of sufficient standing]" to take part in judicial proceedings involving a daughter of Prince William II and a baron. *French chronicle*, pars. 954-72, especially par. 961. See also Jacoby, *La féodalité*, 46-47.

<sup>66</sup> This attitude will be examined below.

<sup>67</sup> On later arrivals, see Jacoby, *La féodalité*, 82-83, 85-86, and Bon, *La Morée*, 168-69, 238-40.

<sup>68</sup> *Crònica de Ramon Muntaner*, ed. E. B. (Barcelona, 1927-51), chs. 244, 261, in fine; *Assises*, art. 111 ("the prince or another lord cannot deny permission to his vassal to leave the principality . . . if he wishes to contract a marriage," obviously in the West); see also Jacoby, *La féodalité*, 41-42. Intermarriage with Greeks is attested, and for the princes of the Morea was obviously prompted by political considerations.

<sup>69</sup> A most suggestive description of building activity is to be found in *French chronicle*. "Li baron dou pays et li autre gentil homme si comencèrent a faire fortresses et habitacions, quy chastel, qui maisons sur sa terre, et changier leurs sournoms et prendre les noms des fortresses

meetings, tournaments, such as the one held in 1304 in Corinth,<sup>70</sup> the historical literature and romances in French read aloud at court gatherings and circulated in the Morea,<sup>71</sup> the frescoes painted on the walls of the castle of Saint-Omer in Thebes, "depicting how the Franks conquered Syria," or in the mansion of the archbishop of Patras, "representing the story of the destruction of Troy,"<sup>72</sup> all these afford a glimpse of the life, values, and outlook of the knightly class of the Morea.

THE MOST OBVIOUS RESULT of the conquest was the abolition of Byzantine imperial rule and administration as well as the replacement of the independent local *archontes* by new masters. The imposition of a group of foreign conquerors on the indigenous population heralded the introduction of a new system of government, with corresponding institutions and a change in the structure of society, both in conformity with feudal concepts and principles. The new social and political regime is clearly reflected in the realm of jurisdiction and taxation, the foremost expressions of political authority; previously vested in the emperor and usurped for a few years by the local *archontes*, they now became the exclusive prerogative of feudal lords. Moreover, the whole structure of society underwent a metamorphosis. It was now divided into two distinct groups: on the one hand, the Latin conquerors and Western immigrants who joined them; on the other, the indigenous Greeks and Slavs. Although religious differences did not constitute a primary factor in the relations between the two groups, they nevertheless became a criterion of basic social status and provided a convenient means of social identification. Latins or Franks, by definition free,<sup>73</sup> were those who recognized the sole authority of Rome in religious and ecclesiastical matters. Although formally subjected to the pope in these fields,<sup>74</sup> the Greeks re-

qu'il faisoient" (pars. 218-19). On the architectural features of these Frankish buildings, see Bon, *La Morée*, 601-84, especially 680; on the social aspect, see Ernst Kirsten, "Die byzantinische Stadt," *Berichte zum XI. Internationalen Byzantinisten-Kongress* (Munich, 1958), 33-34, 39-40. On Frankish feudatories living in cities, see below.

<sup>70</sup> See respectively *Crònica de Ramon Muntaner*, ch. 244, and *French chronicle*, pars. 1015-24; see also Longnon, *L'empire latin*, 192.

<sup>71</sup> The original version of the *Chronicle of Morea* was written in French between 1292 and 1320 and belonged to Bartolomeo II Ghisi, one of the most powerful barons in the Morea, until it was destroyed in 1331 or 1332; a shorter version with interpolations, also in French, was finished between 1341 and 1346; finally, a copy of this version was shipped from the Morea to the West in 1397. See Jacoby, "Quelques considérations," 134-50, 181-89. The inventory of the books left on his death in 1281 by Leonardo da Veroli, chancellor of the principality and trusted counselor of Prince William II, mentions fourteen romances. See Longnon, *L'empire latin*, 213. They were obviously in French, which was also used for the *Assizes of Romania*. See Jacoby, *La féodalité*, 82-88.

<sup>72</sup> The fresco in Thebes is mentioned in *Greek chronicle*, v. 8071-92; see also *French chronicle*, pars. 553-54. It belongs to the period of Nicholas II of Saint-Omer, lord of the castle from 1258 to 1289; see also Longnon, *L'empire latin*, 216. On the fresco of Patras, see Bon, *La Morée*, 452, 597, and n. 4; it is mentioned toward the end of the fourteenth century but cannot be dated.

<sup>73</sup> In opposition to *Grecus*, *Francus* was synonymous with Latin, but it also meant "freeman," hence the verb *affrancare*, to free from the bonds of slavery or servitude.

<sup>74</sup> See Bon, *La Morée*, 89-102, on the Church in the Morea.

mained on the whole faithful to their own Church;<sup>75</sup> with few exceptions, they were considered unfree by the conquerors.

The conquest of the Peloponnesus by the Franks proved to be more difficult than expected, was slow to proceed, and came to a successful end only in 1248. The difficulty of the enterprise and the smallness of their army induced the Frankish leaders to come to terms with the local population whenever possible and to conciliate it by promises formulated in written privileges delivered to the *archontes* or local leaders. The Franks guaranteed the maintenance of the Greek Church and the Greek clergy as well as the implementation of Byzantine law. In the words of the Greek version of the *Chronicle of Morea* they promised "that from now on, no Frank will force us to change our faith for the faith of the Franks, nor our customs and the law of the Romans."<sup>76</sup> Other promises dealt with the integrity of landholding, the preservation of individual privileges, and the retention of the Byzantine fiscal system.<sup>77</sup> Yet absolute continuity was not to be expected. As the conquest proceeded, estates of the Byzantine state, absentee landowners, Greek ecclesiastical institutions, and local *archontes* were confiscated, partitioned, and granted as fiefs to Frankish feudatories.<sup>78</sup> The use of Byzantine law ceased completely in the public sphere and was severely restricted in private matters.<sup>79</sup> The transfer of jurisdiction and taxation into private hands—that is, to the feudal lords—implied the introduction of a new concept of society.

The imposition of a political superstructure imported from the West and lacking any roots in the Peloponnesus arrested the natural evolution of Greek society, which was henceforth subjected to the influence of the conquerors, and even its internal structure was altered. The approach of Western knights to the Greek population depended to a very large extent upon their own political and institutional traditions, on concepts, values, and outlook that had evolved in the feudal society of the West. The Franks conceived society as strongly stratified and based upon a strict hierarchy of personal, private links. Depicting Byzantine society,

<sup>75</sup> A most convincing testimony about the attitude of the Greeks is provided by Marino Sanudo Torsello (*Istoria del Regno di Romania*, in Charles Hopf, *Chroniques gréco-romanes inédites ou peu connues* [Berlin, 1873]), who refers to Cyprus, Crete, Negroponte, Rhodes, other islands, and the Morea. "Benchè detti lochi siano sottoposti al dominio de Franchi e obbidienti alla Chiesa Romana, non dimeno quasi tutto il popolo è greco e inclina a quella setta, e il cuor loro è volto alle cose greche, e quando potessero mostrarlo liberamente, lo farianno" (p. 143). Although the *Istoria* was written between 1328 and 1333, more than a century after the conquest, this description no doubt reflects a permanent situation. Sanudo expressed the same opinion in a letter written on April 10, 1330, published by Friedrich Kunstmann, "Studien über Marino Sanudo den Aelteren, mit einen Anhang seiner ungedruckten Briefe," *Königliche bayerische Akademie der Wissenschaften, Historische Klasse, Abhandlungen*, (Munich, 1855), 7: 777. There is, however, evidence that some Greeks joined the Roman Church in the thirteenth or fourteenth century. See below. The Slavs were in the same category as the Greeks.

<sup>76</sup> *Greek chronicle*, v. 2093–95.

<sup>77</sup> See Jacoby, "Les archontes," 430–31.

<sup>78</sup> *Ibid.*, 423, 426–27, 441–42.

<sup>79</sup> See Jacoby, *La féodalité*, 34–38.

Western chroniclers of the early thirteenth century often speak of "fealty and homage" performed by Greeks to their emperor, implying thereby the existence in the Empire of a political and social structure similar to that of the feudal West.<sup>80</sup> Their description reflects a complete lack of understanding on the part of the Latins for societies other than their own. It is therefore not surprising that Frankish knights viewed the Greek society of the Morea as basically divided into two classes: on the one hand, the *archontes*, considered as free, on the other, the *villani*, or villains, subjected to the authority and jurisdiction of their lords and enjoying only severely limited personal freedom and legal capacity.<sup>81</sup> Free Greeks who did not belong to the class of *archontes* are seldom mentioned; numerically few, they represented a marginal element within the Greek society of Frankish Morea.<sup>82</sup>

The Franks not only perceived Greek society as divided into two classes but also considered that each of these, again in conformity with Frankish concepts, should be governed by its own set of laws. Characteristically the Franks translated social and economic realities into legal terms, and what had been, under Byzantine rule, a relatively "open" Greek society became a highly stratified system of sociolegal classes, the social status of the Greeks becoming hereditary. Crossing the basic lines of divisions became extremely rare and difficult and presumed a legal procedure to achieve it. Neither wealth nor education were of any weight, and upward mobility was solely dependent upon the favor of the Frankish princes and barons.

In spite of the cleavage between Latins and Greeks, the *archontes* gradually achieved some measure of integration within the ranks of the Moreot feudatories, the first step having already been taken at the time of the conquest. The agreements between the conquerors and *archontes* contained, among other clauses, provisions for homage and an oath of fealty performed by the *archontes* to the first leader of the conquerors, William of Champlitte, and, later, to his successors, the princes of Morea. As vassals the *archontes* were included in the class of feudatories owing simple homage, the lowest stratum in the feudal hierarchy, together with the Frankish mounted serjeants who were not of noble descent.<sup>83</sup> The first stage of integration therefore had a limited scope. It applied to the *archontes* only on a personal level and did not imply any grant of fiefs; it had a solely legal character but did not result in social integration within the nobility. Nor did it involve any change for the *archontes*

<sup>80</sup> See Jacoby, "Les archontes," 439-40.

<sup>81</sup> *Assises*, arts. 178, 194; art. 198 speaks of the *Griego villan*, to be distinguished from the Greek, who is an *archon*.

<sup>82</sup> For the time being, see David Jacoby, "Un aspect de la fiscalité vénitienne dans le Péloponnèse aux XIV<sup>e</sup> et XV<sup>e</sup> siècles: le 'zovaticum'." *Travaux et Mémoires*, 1 (1965): 419 and n. 81.

<sup>83</sup> See above, n. 60. On *archontes* who achieved a higher status, see below.

in the status of their hereditary estates, the transmission and alienation of which were governed by Byzantine law, as before the conquest.<sup>84</sup>

The integration of the *archontes* within the ranks of the feudal hierarchy gradually proceeded beyond the legal and personal aspects already described, and a new stage was reached by the middle of the thirteenth century. That political as well as military considerations account for this process is perfectly illustrated by Moreot chronicles and Angevin documents. In 1248, after Monemvasia in the southern Peloponnesus had surrendered to him, Prince William II granted fiefs to three *archontes*, members of the three most influential families of this city, who thereby enlarged their landed wealth.<sup>85</sup> Grants of this nature also took place toward the end of 1261 or in the first months of the following year. When the Latin emperor of Constantinople, Baldwin II, traveled to the West after his capital had been recaptured by Byzantine forces, he stayed a few months in the Morea. Some of his vassals, among them Greek *archontes*, settled in the principality and were enfeoffed by Prince William II.<sup>86</sup> Prior to his death in 1278 the prince endowed the grandfather of John Sideros with feudal land.<sup>87</sup>

About the same time a new factor of integration appears. During the war waged in 1262 by Geoffroy de Briel, baron of Karytaina, against Byzantine forces in the southern Peloponnesus, this Frankish lord, using a stratagem suggested to him by the Greek warriors in his service, managed to vanquish the enemy. After achieving victory Geoffroy "gave to his Greeks many fine presents in land and other kind, and from the noblest amongst them he made knights." According to the story there was an intimate relationship between the Frankish baron and Greek members of his household: "he had nourished and brought them up," and they were his vassals,<sup>88</sup> obviously by simple homage, as was customary.<sup>89</sup> He endowed them with feudal tenements, which was not unusual by that time. Special attention, however, must be given to his attitude toward the "noblest amongst" the Greeks, who were no doubt of higher social status and probably belonged to the class of *archontes*. They were dubbed by their lord and, as a result, became knights in the same way as

<sup>84</sup> *The Assizes of Romania* devote four articles to the status of the *archontes* and their estates, arts. 71, 138, 178, 194. See also Jacoby, "Les archontes," 451-63.

<sup>85</sup> *Greek chronicle*, v. 2955; Jacoby, "Les archontes," 470.

<sup>86</sup> *French chronicle*, par. 87. For the approximate date of the enfeoffments, see Jacoby, "Quelques considérations," 165 and nn. 150-51.

<sup>87</sup> See below, n. 98.

<sup>88</sup> *Aragonese chronicle*, pars. 311-31, especially 313, 319, 331 ("et de los mas nobles fizo cavaleros"). The dating by the editor of this version ("1264") is erroneous and should be corrected by taking into account the sequence of events following the return of Prince William II to the Morea (see above, n. 86), as related by *Aragonese chronicle*, pars. 308-12; the episode took place before the arrival of a second Byzantine army to the Peloponnesus in 1263, on which see Bon, *La Morée*, 129.

<sup>89</sup> See above, p. 887 and n. 60.



the Franks.<sup>90</sup> The ceremony of dubbing sanctioned their firm and definitive integration within the ranks of the feudal nobility; once granted to a Greek, the status of knight obviously became hereditary. On the basis of fourteenth-century sources it seems that Greek knights henceforth bore the title *dominus*, or its French or Italian equivalents,<sup>91</sup> and rose to the rank of lieges by performing the appropriate rite prior to the investiture of their new fiefs. Added to vassalage and fiefholding, already fused, the knighthood granted by Geoffroy de Briel to some of his Greeks completed the process of social integration within the feudal nobility. And although no such evidence is available for vassals of the princes in the thirteenth century, one may assume that several of them enjoyed the same privilege.

It should be stressed that this integration was not achieved by all *archontes*, nor was it restricted to members of their class. In the fourteenth century two different groups of Greeks achieved integration in varying degrees. The largest group consisted, to be sure, of Greek feudatories, mostly *archontes*, who had performed simple homage; although feudatories, they did not become members of the knightly class. Significantly, they appear together with Frankish squires or mounted serjeants, who had not been dubbed, in the host assembled in 1304 by Nicholas of Saint-Omer to wage war against the Greeks of Epirus.<sup>92</sup> Besides *archontes* one finds in 1354 other Greeks among the feudatories of simple homage, such as the son of a *papas*, or Greek priest, and a *magister*, both undoubtedly of lowly origin and generally considered unfree by the Franks.<sup>93</sup> This clearly emphasizes that social promotion involving the crossing of class boundaries was entirely dependent, in Frankish Morea, upon the favor displayed by Frankish barons, princes, or representatives of princes. The integration of Greeks, mostly *archontes*, within the lowest stratum of the feudal hierarchy, initiated by the middle of the thirteenth century, seems to have gained considerable impetus thereafter. This phenomenon was no doubt concomitant with a decline in the number of Frankish feudatories and the growing need for military forces and administrative personnel.<sup>94</sup>

<sup>90</sup> See Jacoby, "Les archontes," 470-71.

<sup>91</sup> Jean Longnon and Peter Topping, *Documents sur le régime des terres dans la principauté de Morée au XIV<sup>e</sup> siècle* (Paris, 1969), respectively 21, l. 11 (in a document dated 1336); 128, ll. 5-6 (of 1354 or later): "per messere Johanni Misito," who was a liege (see below, p. 895 and n. 101), "et altri nobili de lo paese"; "per dominum Stephanum Cutrullum, militem" (on whom see below).

<sup>92</sup> *French chronicle*, par. 885. Among eighty-nine cavalrymen, there were "xiiij chevaliers adoubés, et li autres estoient escuier et gentil homme dou pays et tramontains"; "gentil homme dou pays" refers to Greek *archontes*. See also Jacoby, "Les archontes," 471. For later evidence on such *archontes*, see below.

<sup>93</sup> "Magister Manollus Vorkas" and "Theodorus Papa Stamatopulus." Longnon and Topping, *Documents*, 101, ll. 38-39. When "papa" appears between the first name and the name of the father, it should read "pape" and is related to the latter; see, e.g., "Ebreto Pape Leonis," "Xena" (a woman), "Pape Chimacopulo." *Ibid.*, 49, l. 3; 60, l. 41.

<sup>94</sup> For this process, see below. That most Greek feudatories were indeed *archontes* is illustrated

A second, smaller group of Greek feudatories consisted of those who were granted knighthood by barons, princes, or representatives of the latter. These feudatories as a rule held the rank of lieges,<sup>95</sup> thereby achieving integration within the feudal nobility. Besides *archontes*, one also finds among them the son of a *papas*, liable to six months a year of mounted service, according to a source dated 1354.<sup>96</sup> His social ascent is particularly impressive and does not seem to have been an isolated case. Although it is impossible to know how many of the Greek knights were not *archontes*, the number must have remained small. As the status of Greek knights was hereditary, it is not surprising to find some families active throughout the fourteenth century, but only a few attained prominent positions in the feudal elite and played a dominant part in the history of Frankish Morea. In spite of their power, landed wealth, and prestige in the principality none of them gained access to the baronial group. Only Franks and a few Italians became barons, and none of their families intermarried with Greek lieges.

The position of Greek feudatories as members of the social and political elite of the principality is well documented. Among "the ten best men of the country" of Chalandritsa in 1316, one finds two Latin lieges and three Greeks, presumably feudatories of simple homage.<sup>97</sup> John Sideros, scion of a family of Greek feudatories, was the grandson of a man who had received feudal lands from Prince William II prior to 1278. When a group of barons decided in 1341 to hand over the principality to Byzantine rule, Sideros, perhaps because of his knowledge of the Greek language, was chosen as one of the two emissaries who submitted the plan to Emperor John VI Kantakouzenos.<sup>98</sup> George and Basil Galentini, sons of a Greek knight, along with the two sons of the powerful baron Centurione I Zaccaria and other "noblemen" in 1374 asked the bailiff Francesco de San Severino to grant them knighthood, while the lists of feudatories who guaranteed the agreements concluded with Venice in 1387 and 1396 on behalf of the principality include several Greek lieges.<sup>99</sup>

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by the *Assizes of Romania*, arts. 71, 138, 178, 194; this treatise refers only to them when discussing the position of Greeks in the context of the feudal elite.

<sup>95</sup> See the list of Latin and Greek feudatories in Longnon and Topping, *Documents*, 73, ll. 1-12. The first two are lieges bearing the titles "meser" and "dominus" (on which see above, p. 893 and n. 91), whereas the others are feudatories of simple homage. The correspondence between liege homage and knighthood has already been emphasized above.

<sup>96</sup> "Dominus Theoderus Papa Chyriacopulos, tenetur servire sex mensibus cum serviciis stivoriis." *Ibid.*, 101, ll. 33-34. On the *stivoria*, see Jacoby, *La féodalité*, 135-37.

<sup>97</sup> Johan Janopolo, Raynalt Jalomati, and Mamallera Vazilaqui are Greeks. *Aragonese chronicle*, sec. 590. The two Latin lieges are entitled "micer," on which see above, nn. 91, 95.

<sup>98</sup> By 1341, sixty-three years had elapsed since the death of William II. It does not seem likely that the father of John Sideros was the beneficiary of the grant, as suggested by Hopf (*Geschichte*, 1: 433 a) and Bon (*La Morée*, 208, where "1302" should be replaced by "1304," and 242); see also *ibid.*, 212-13.

<sup>99</sup> *Aragonese chronicle*, pars. 718-21; see also Jacoby, "Les archontes," 474.

A most spectacular rise was achieved by two families, whose members took advantage of their position as high-ranking officers in the princely administration. The knight Stephen Cutrullus, or Koutroules, was in 1336 *protovestiarius* of the principality and as such handed out fiefs on behalf of the prince, controlled their content, and sold the products of the princely demesne. His son Nicholas, active since 1354 at least, was one of the most powerful feudatories in 1379, while John, son of Nicholas, was still a squire (*scudarius*) in 1375, having not yet been dubbed.<sup>100</sup> John Misito was granted several fiefs between 1313 and 1316, which were confirmed in 1324 by Prince Jean de Gravina when Misito was captain of Kalamata on the prince's behalf. John Misito was succeeded three years later in his fiefs by his son Nicholas, who died in 1344 and appears among the most influential men of the principality at that time.<sup>101</sup> The third member of this dynasty of Greek lieges was John II, one of the mightiest landlords of the Morea in 1377, who died before 1391.<sup>102</sup>

The knight's fief, or *feudum nobile*, of the Morea provided as a rule a revenue of 1,000 hyperpers and the standard fief of serjeanty half this income; feudatories holding such fiefs were liable to mounted service during the whole year.<sup>103</sup> Whether vassals of princes or of Frankish lords, most Greek feudatories were endowed with small fiefs, for which they had to perform only a small amount of military service. A servant of Catherine of Valois, mother of Prince Philip of Taranto, received together with his wife, prior to 1338, a fief supplying a revenue of twelve gold ounces or 189 hyperpers, with an obligation to provide the corresponding service, two months a year.<sup>104</sup> In 1354 one of the Greek feudatories of simple homage in Krestena was liable for one and a half months of mounted service, another for three months, and a third for a whole year; Nicholas Cutrullus, a knight, owed six months.<sup>105</sup> In several other cases, the small feudal tenements held by Greeks were obviously parts of

<sup>100</sup> Longnon and Topping, *Documents*, 21, ll. 11–12; and 45, l. 8, mention Stephen in 1336 and 1337; 73, l. 3; 150, l. 10; and 198, l. 6, refer to Nicholas, respectively in 1354, 1361, and 1379; see also Jacoby, "Les archontes," 472 and nn. 261, 265, as well as 473–74. On the functions of the *protovestiarius*, see Longnon, *L'empire latin*, 198, 205. The unpublished document mentioned in Jacoby, "Les archontes," 474 n. 271 (in fine), refers to John in 1375 and not in 1405, the year it was drafted. On other Greek squires, see *Aragonese chronicle*, pars. 718–21, and Jacoby, "Les archontes," 474.

<sup>101</sup> See Hopf, *Geschichte*, 1: 423 b, 435 b; Bon, *La Morée*, 204–05, 213–14, 242, 427 (where "Florent de Hainaut" should be replaced by "Louis de Bourgogne").

<sup>102</sup> Longnon and Topping, *Documents*, 127, l. 28–30; 149, l. 22–150, l. 9 (as liege of Prince Robert of Taranto); 198, l. 16; 214, l. 22. See also Bon, *La Morée*, 242, 252, 253 n. 1, 275, and Jacoby, "Les archontes," 473.

<sup>103</sup> See Jacoby, "Les archontes," 449–51, and *Aragonese chronicle*, par. 139; see also above, n. 55.

<sup>104</sup> Longnon and Topping, *Documents*, 58, ll. 8–10; comment in Jacoby, "Les archontes," 472–73. On the rate of exchange of the ounce, see Longnon and Topping, *Documents*, 21 n. 8; on the ratio of the tournois to the sterling and hyperper (1:4:80), see Jacoby, "Un régime de coseigneurie," 122 n. 7.

<sup>105</sup> Longnon and Topping, *Documents*, 73, ll. 6, 9–11. For other small fiefs of Greek feudatories, see *ibid.*, 95, ll. 18–22; 101, ll. 33–36; on Nicholas Cutrullus, see also above.

larger grants comprising scattered plots of land.<sup>106</sup> The fragmentary state of our documentation does not enable a definitive assessment of the income provided by the fiefs of Greek feudatories, especially of the lieges, who were entitled to hold more than one fief and to have holdings from several lords.<sup>107</sup> It seems obvious nevertheless that many of the fiefs held by Greek feudatories were insufficient to supply a livelihood to their holders. One may therefore safely assume that several of the latter had additional sources of income, primarily hereditary estates.

This was to raise many intricate problems, as hereditary estates held by Greeks were governed by Byzantine law. Compared with fiefs they enjoyed preferential status; they were what medieval Western jurists would have called allodial lands. They could be divided among heirs into equal parts, in true Byzantine fashion, whereas the fief could not be partitioned. Many of these estates owed no military service at all, and in cases where such service was to be performed, it could be evaded; indeed, at times it was difficult, if not impossible, to assess the amount of that service, because of the continuous process of partition.<sup>108</sup> Moreover, it was to be expected that, in some cases at least, Greeks holding fiefs would try to assimilate them to their hereditary estates, in order to escape the obligations and restrictions implied in the status of their feudal land. One can, then, easily understand the stress laid upon a clear-cut distinction between these two categories of landholding, a distinction that is once again to be found in the *Assizes of Romania*. It is not surprising that among the Frankish feudatories, especially of the lower rank, to whom most of the *archontes* and Greek feudatories were assimilated, a certain antagonism to the *archontes* should have built up in connection with the holding of nonfeudal land. These Frankish feudatories urged the imposition of military service on all land held in the principality by members of the feudatory class, whether Franks or Greeks, regardless of the legal regime by which this land was governed. The compiler of the *Assizes of Romania* expresses his support for this trend, but there is no evidence that it gained the upper hand. There can be no doubt, however, that some tension prevailed between Greek and Latin feu-

<sup>106</sup> See, for example, documents drafted in 1337 and 1338 that reflect a situation prior to these dates. *Ibid.*, 43, l. 27-44, l. 4; 45, ll. 10-22; 62, ll. 12-31, which should be compared with the parallel data provided by a document of 1354, in *ibid.*, 113, l. 22-114, l. 20.

<sup>107</sup> See *Assises*, arts. 34, 46, 63, 68, 99.

<sup>108</sup> *Greek chronicle*, v. 1644-48, relates that it had been agreed upon at the time of the conquest that *archontes* of certain areas would perform military service according to the size of their estates. See Jacoby, "Les archontes," 430-31. Among the vassals of Niccolò Acciaiuoli, one Greek was liable to five months of mounted service "pro suo de redetago," i.e., *hereditagio*, or hereditary estate, while another owed six months of a serjeant's service: "tenetur de uno scutiferu mesibus [*sic!*] sex per anno redetago suo"; at Glyky, Goti Murmuru owed only one month of unspecified service for the same reason. Longnon and Topping, *Documents*, 77, ll. 9-12; 83, l. 5. Goti Murmuru, or Mourmoures, was probably a member of the well-known family of officials, on which see *ibid.*, 298, s.v. Murmuru. On the problems relating to the hereditary estates of the *archontes* and other Greek feudatories, see Jacoby, "Les archontes," 451-58; see also 430-31, 442-45.

datories in the third and fourth decades of the fourteenth century, the period in which the *Assizes* were compiled.<sup>109</sup>

In spite of this tension the integration of Greek feudatories within the feudal class of Morea proceeded even further than I have indicated. The most striking expression of this phenomenon is to be found in the second half of the fourteenth century; it is embodied in the Greek version of the *Chronicle of Morea*. The original version of this chronicle, no longer extant, was, as we have seen, written in French approximately between 1292 and 1320. In the true fashion of the *chansons de geste* it gave a glowing account of the epic of conquest, linking events in the Morea during the early thirteenth century with the First Crusade and the conquest of Jerusalem. The chronicle glorified the deeds of the conquerors and their descendants, emphasizing their military superiority and high morality. The author displayed an intense interest in judicial procedure as well as a thorough dislike and contempt for the Greeks. The original *Chronicle of Morea* was a typical product of the knightly class of the Morea; it was clearly intended for the members of this class and was to be read aloud when they assembled.<sup>110</sup>

The Greek version of the *Chronicle of Morea* was composed at a later date on the basis of a shorter French version and various oral traditions. Evidence seems to indicate that its author was a Greek feudatory, a member of the court of Erard III, lord of Arkadia in the southern Peloponnese.<sup>111</sup> Being a Greek he felt the need to emphasize his hatred against the enemies of Frankish Morea, but significantly refrained from attacking the Greeks of the principality. The anti-Greek tendency of the French version of the *Chronicle*, heightened to a considerable extent in the Greek version,<sup>112</sup> is directed exclusively against the Greeks of Byzantium and of the despotate of Epirus: "Who will put faith in them, believe their oath, since they do not respect God nor love their ruler? They do not love each other except with guile."<sup>113</sup> From his attacks against the Orthodox Greeks, whom he accuses of being schismatics,<sup>114</sup> one may infer that the author accepted the subjection of the Greek Church to the pope, as established in the Morea at the time of the conquest, and therefore, in the religious and ecclesiastical sphere, clearly distinguished the Greeks of the principality from other Greeks.

It is apparent, then, that the author of the Greek version espoused entirely the cause and ideals of the conquerors, considering himself a Frank in the same way as did the feudatories of Western descent. When he

<sup>109</sup> For the evidence, see Jacoby, *ibid.*, 455-56, 458-59.

<sup>110</sup> For its dating and characteristics, see Jacoby, "Quelques considérations," 181-87; see also 134-35, 136-38.

<sup>111</sup> See *ibid.*, 140, 155, and especially 157.

<sup>112</sup> See especially the additions due to the author of the *Greek chronicle*, v. 724-30, 754-841, 1260-62, 3932-37, 3940, 3974-75.

<sup>113</sup> *Ibid.*, v. 728-30.

<sup>114</sup> *Ibid.*, v. 798-800.

refers to the Latins camping outside Constantinople in 1203, he speaks of "our people,"<sup>115</sup> which implies, on his part, full identification with the interests, values, and attitudes, even with the past of the Frankish feudatories. To be sure, the author did not express only personal views, nor were his views put forward solely in order to impress them upon other Greeks. The Greek version of the *Chronicle of Morea* was undoubtedly intended for Greek-speaking feudatories who identified themselves with the Franks in the same way the author did. The political verse in which it was written suited the taste of a Greek public; the language, a Greek strongly tainted by French and Italian influences and saturated with feudal terminology, reflects the Greek dialect spoken in the Morea in the fourteenth century.<sup>116</sup> At the same time that the Greek version of the *Chronicle of Morea* illustrates the attitudes and outlook of at least a section of the Greek *archontes* and feudatories, it also measures the extent to which they had achieved integration within the class of feudatories and points to their growing class-consciousness.

Some Greeks even went a step further. In 1245 Manuel Mourmouras, whose family was to play an important role in the administration of Frankish Morea,<sup>117</sup> built a church at his own expense in the area of Kranidion, in Argolis; its dedicatory inscription, besides mentioning this Greek landlord, seems to indicate that the latter had joined the Roman Church.<sup>118</sup> Similarly, a Greek epitaph found at Kalamata in the southern Peloponnesus and dated 1354, according to the Christian era used by the Franks but without any Byzantine dating, may have been ordered by a Greek who embraced the Roman faith.<sup>119</sup> Such cases, prompted by the desire to achieve full assimilation to the Franks, must have remained quite exceptional, most Greeks remaining faithful to their own Church. Indeed Latins started to adhere to the Greek rite, a convincing testimony to the weakness of the Latin clergy, which was bitterly denounced by Pope John XXII in 1322.<sup>120</sup>

Although achieved on a legal, social, and at times even religious level, the integration of Greeks was not complete. To be sure, many Greeks, especially feudatories, were bilingual. The very existence of a Greek version of the *Chronicle of Morea* is nevertheless significant, as it emphasizes that a gap persisted between Franks and Greeks in the cultural sphere. The same holds true in the religious field, although some

<sup>115</sup> *Greek chronicle*, v. 636.

<sup>116</sup> See Jacoby, "Quelques considérations," 157-58, 187.

<sup>117</sup> See above, n. 108, in fine.

<sup>118</sup> See Georgios Soteriou, "He Hagia Trias tou Kranidiou," *Epeteris tes hetaireas byzantinon spoudon*, 3 (1926): 193-205 (in Greek), and Bon, *La Morée*, 115.

<sup>119</sup> See Nikos A. Bees, "Christianikai epigraphai Messenias," *Deltion tes historikes kai ethnologikes hetaireas tes Hellados*, 6 (1904): 375 (in Greek), and Bon, *La Morée*, 591 n. 3, in fine.

<sup>120</sup> Caesar Baronius, *Annales ecclesiastici*, 24 (Bar-le-Duc, 1872): 187-88, anno 1322, par. 48 (Oct. 1, 1322); the same letter also mentions Greeks attending services of the Latin Church. On the Greeks, see also above, n. 75.



Greeks had joined the Roman Church. Relying on an earlier, presumably oral, tradition,<sup>121</sup> the author of the Greek version relates that the conquerors had promised that "no Frank will force us to change our faith for the faith of the Franks"<sup>122</sup>—the use of "us" for those who belong to the Greek Church in itself being highly significant. Many Greek feudatories resided in urban centers, while Frankish knights lived mostly in castles; intermarriage between the two groups is seldom attested and must have been rare in any event. All these factors evidently prevented social fusion of the two groups, a fact illustrated by a most revealing remark in the *Greek Chronicle*. While following the French version, which speaks of the arrogance of Gautier V de Brienne, duke of Athens, the author of the Greek version points out that this "is a trait of the Franks."<sup>123</sup> In spite of his strong identification with the Frankish feudatories, he viewed himself as part of a different social group, and such presumably was the feeling of most *archontes* and Greek feudatories.

CONSIDERING THE ATTITUDES and values common to the Frankish knights at the time of the conquest, the integration of Greeks within the class of feudatories seems at first glance surprising, but the conjunction of several factors explains how this phenomenon was brought about: the class-consciousness and self-confidence of the Frankish knights; their approach to the *archontes*; their practical needs; and, finally, the eagerness of the Greeks to achieve integration. The pattern of integration can be safely reconstructed.

Although imbued with a powerful class-consciousness, the Frankish feudal elite was willing to a certain extent to loosen its rigid system of social and legal stratification, so as to allow for the entry of Greeks within the lower rank of the feudatories. The Greek *archontes* could easily be viewed by the Frankish conquerors as Greek counterparts. Their traditional standing in Greek society; their leadership, combined with the exercise of a certain measure of authority; their style of life, different to some extent from that of other Greeks—all these seemed familiar to the Frankish knights. Besides, many *archontes* had undoubtedly been granted fiscal privileges by the emperors prior to the conquest. In the West, as we have seen, exemption from regular taxation on the whole distinguished the knightly class from other classes of society.

Therefore, when political, administrative, and military needs made it imperative Frankish princes and barons encouraged the integration of Greeks into the class of feudatories. The first stage was restricted to a

<sup>121</sup> On oral traditions incorporated in the Greek version, see Jacoby, "Quelques considérations," 153, 155.

<sup>122</sup> *Greek chronicle*, v. 2093-94.

<sup>123</sup> Compare *French chronicle*, par. 500, and *Greek chronicle*, v. 7263-7300, especially 7290, loosely translated here.

personal and legal aspect; on the whole, it was a convenient device, in conformity with the attitudes and outlook of the conquerors, to ensure the loyalty of the leaders of the local Greek and Slav population as well as the fruitful cooperation of Greek *archontes* with the conquerors in the administration of the principality. Even in the early years of Frankish rule, prior to 1210, several *archontes* assisted Frankish leaders as they carved out fiefs in the conquered areas:<sup>124</sup> the use of Byzantine land lists in this procedure depended upon the knowledge of the Greek language and of the intricate Byzantine fiscal terminology. As late as the fourteenth century fiscal documents were still drafted in Greek,<sup>125</sup> which partly accounts for the importance of the Greeks among the administrative personnel of the principality.<sup>126</sup> Integration grew in scope and intensity when the military position of Frankish Morea worsened in the sixties of the thirteenth century. Greeks, who started to display interest in the process, were obviously prompted more by social considerations than by material concerns such as the acquisition of fiefs, which were mostly small. Especially if he was dubbed, the Greek feudatory gained access to a new, superior status in society, adorned with special prestige; in relation to other Greeks, he greatly enhanced his social position. The pressure on the part of Greeks to receive knighthood must have gradually increased in the fourteenth century, and, correspondingly, so did the willingness of the princes and barons to grant it.<sup>127</sup> In view of the precarious political and military position of the Franks and the constant pressure exerted by Byzantium, the full cooperation of the Greeks was essential. Integration in the long run implied acceptance of the standards, values, and attitudes of the Frankish knights. The eagerness to achieve complete identification with this class is convincingly illustrated by the Greek version of the *Chronicle of Morea*.

It should be stressed that integration within the ranks of feudatories and, more specifically, within the knightly class was not restricted to Greeks and should be viewed in a wider context. The same general considerations that prompted the Frankish feudal elite to display a favorable attitude toward the *archontes* proved beneficial to Slav leaders<sup>128</sup>

<sup>124</sup> See *French chronicle*, pars. 107, 120, and *Greek chronicle*, v. 1641–50, 1830–35. On the circumstances, see Jacoby, “Les archontes,” 441–42, and *La féodalité*, 54, 223–25.

<sup>125</sup> “In quodam practico in greca scriptura scripto, facto per dictum Johannem Murmurum” in 1337. Longnon and Topping, *Documents*, 52, ll. 14–15. “E o facti li inventarii in greco,” in 1361. *Ibid.*, 147, l. 9.

<sup>126</sup> See above on the families Murmurus, Cutrullus, and Misito; in 1297 Vassilopoulos appears as *protovestiarius*, on whose functions see above, n. 100. Nikolakos of Patras was governor of the castle of St. George of Skorta in 1319 or 1320. *French chronicle*, par. 829, and p. 404, the latter confirmed by *Aragonese chronicle*, par. 647; see also below, n. 143. Many minor officials were Greeks.

<sup>127</sup> Which explains the growing number of Greek knights in fourteenth-century documents. See Bon, *La Morée*, 238–39, 260 n. 1, 266 n. 3. The same trend is perceptible in regard to Latins; Egidius de Leonessa, a *physician*, is granted a vineyard “in feudum et ligiam” in 1397. Text in Ernst Gerland, *Neue Quellen zur Geschichte des lateinischen Erzbistums Patras* (Leipzig, 1903), 185–86, doc. no. 7, and see Jacoby, *La féodalité*, 179–81.

<sup>128</sup> *Greek chronicle*, v. 3008–31, provides details about the submission of the Melings and the homage of their leaders to William II.

and even to Turks. When in 1263 Turkish mercenaries remained in Frankish Morea, William II ordered them to be baptized, and thereafter the prince made two of their leaders knights, endowed them with fiefs, and gave them wives—at least one of whom was a Frankish noblewoman, presumably of low rank.<sup>129</sup> Pressing—and well attested—military needs at this period and the establishment of Turkish troops in the principality account for these steps.<sup>130</sup> Far more important was the trend, beginning in the second half of the thirteenth century, toward the integration of Italian non-nobles, several of whom served in the princely administration. Leonardo da Veroli, for example, chancellor of the principality during the reign of William II, married the daughter of a Frankish baron.<sup>131</sup> When princes granted knighthood and fiefs to Italian merchants and bankers they did so primarily for economic reasons. Pietrono da Siena, a resident of Glarentsa, the most important harbor of Frankish Morea, was a liege of the prince since the sixties of the thirteenth century. Other Sienese and Florentines, among them members of the Acciaiuoli family, were endowed with the same status and fiefs in the late thirteenth and in the first half of the fourteenth centuries, presumably in return for the financial help they had extended to the princes; they in turn were joined by several Genoese and Venetians.<sup>132</sup>

The entrance of growing numbers of Greeks and Italians into the ranks of feudatories, especially of the knights, inevitably affected the nature of Frankish society, and of the knightly class in particular. Many Greek and presumably all Italian feudatories of non-noble descent with few exceptions<sup>133</sup> resided in urban centers, the Italians concentrating in the maritime cities of the Morea.<sup>134</sup> The extension of Byzantine domination in the Peloponnesus, initiated in 1262, gradually reduced the rural areas under the rule of the principality and prompted newly arrived Italians to join their brethren in the cities.<sup>135</sup> Greek lieges, several of them active in the administration of the principality, mingled with Frankish knights. At the same time Italian merchants and bankers combined their economic activity with a newly acquired knightly status, violating thereby the code of chivalry so cherished by Frankish knights. No con-

<sup>129</sup> *French chronicle*, par. 397; *Greek chronicle*, v. 5730-38; *Aragonese chronicle*, par. 363.

<sup>130</sup> On the area in which they settled, see Bon, *La Morée*, 337-38.

<sup>131</sup> On Leonardo, see Bon, *La Morée*, 127, 128, 149, 156, 160, and above, n. 71.

<sup>132</sup> On Italians in the Morea, see Jacoby, *La féodalité*, 84-85; see also 227, 234; the Venetians endowed with fiefs were not always resident in the Morea.

<sup>133</sup> Such as those who became barons, the Ghisi and Zaccaria.

<sup>134</sup> It should be emphasized that Frankish knights and barons, although resident in rural areas, sometimes had houses within the cities. Lise des Quartiers had one in Glarentsa prior to 1337. Longnon and Topping, *Documents*, 48, l. 29; see also Bon, *La Morée*, 323-24. In 1375 Erard III Mavros, lord of Arkadia, had a house in Venetian Modon. See Jacoby, "Quelques considérations," 149 n. 68. A princely residence existed in Andravida, considered the capital of Frankish Morea, as well as in other cities. See Bon, *La Morée*, 677; some of the vassals attached to his court may have settled in them permanently.

<sup>135</sup> Examples for 1353, 1414, and 1429 are found in Gerland, *Neue Quellen*, 155, ll. 11-15; 199, ll. 4-6; 210, ll. 16-18. Italian knights appear in a list of 1344. Bon, *La Morée*, 238-39.

tradition between the two was perceived in Italian cities. By 1320, therefore, as a result of the integration of Greeks and Italians within its ranks, the knightly class of Morea had already lost its homogeneous character.<sup>136</sup>

The dual process of social integration and subjective identification of the Greek *archontes* and feudatories with the Frankish feudatories also had an important effect on the Greek society of Frankish Morea. It stripped this society of a leadership willing to favor Byzantine expansion in the Peloponnesus. Moreover, as the Greek Church lacked the support of the *archontes*, it could not become the backbone of opposition to foreign rule, as it did in Venetian Crete at the same period.<sup>137</sup> The interests of the Greek feudatories obviously lay with the Franks, which explains why the Greeks of Frankish Morea on the whole remained as faithful to their rulers and fought as bravely in their armies as they did in 1264 and 1304. Only two instances of regional revolts on their part have been recorded during the two centuries of Latin rule. With the reappearance of Byzantine forces in the Peloponnesus in 1262, when it seemed as if the Empire would regain its lost territories in the peninsula, unrest was generated within the Greek and Slav population. The Greek inhabitants of Lacedemonia left their city for Byzantine Mistra, and the Greeks of Skorta, in the western Peloponnesus, rose in arms against their rulers. After the Byzantine armies had been repelled and the revolt crushed, Greeks remained in peace for about forty years.<sup>138</sup> Those of Skorta rebelled again in 1304 under the leadership of a Greek priest and several *archontes* when Prince Philip of Savoy imposed taxes on the *archontes*, a step that was rightly considered a breach of faith. The *archontes* of Skorta had been promised fiscal exemption, presumably at the time of the conquest; moreover, the prince's action also deprived them of their privileged status and of the benefits of integration into the class of feudatories.<sup>139</sup> Other revolts against the Franks broke out only in areas inhabited by Slav populations, which had retained their firm tribal structure, traditional leadership, and spirit of independence. Such was the case of the Slavs of Tsaconia and the Taygetos Mountains who participated in the rising of 1263–64<sup>140</sup> and of others who seized Kalamata in 1292 or 1293.<sup>141</sup> Two cases of treason should also be mentioned in this connection. In 1296 a Greek who wished to avenge an outrage inflicted on him by a Frankish

<sup>136</sup> See Jacoby, *La féodalité*, 86.

<sup>137</sup> On Crete, see Freddy Thiriet, *La Romanie vénitienne au moyen âge. Le développement et l'exploitation du domaine colonial vénitien (XII<sup>e</sup>–XV<sup>e</sup> siècles)* (Paris, 1959), 288–93, 301–02, 403–06, 429–33.

<sup>138</sup> See Bon, *La Morée*, 129–34, and on Skorta, 363–406.

<sup>139</sup> *French chronicle*, pars. 920–51, especially 921–23 and 950; see also Bon, *La Morée*, 178, and, for the time of the conquest, Jacoby, "Les archontes," 430–31 and n. 50, where the date should be corrected.

<sup>140</sup> See Bon, *La Morée*, 129–34, 498–502, on these Slavs.

<sup>141</sup> *French chronicle*, pars. 693–745; see also Bon, *La Morée*, 168, 505–07; the erroneous date "1295" on 506 should be corrected.

knight bribed officials of the prince of the Morea, inducing them to deliver the castle of St. George to Byzantium.<sup>142</sup> Then, after the castle had been returned to Frankish rule, it was once again handed over to Byzantium, in 1319 or 1320, by its venal commander, a Greek official of the prince.<sup>143</sup> On both occasions, personal reasons prompted these Greeks to betray their ruler. These cases obviously do not reflect the attitude of Greek society at large. Furthermore, no Greek revolt has been recorded for the principality of the Morea after 1304, precisely the period of the impressive integration of Greek feudatories. The encounter of Western conquerors and local population had produced profound changes both in the knightly class and in the Greek society of the Morea.

THE ENCOUNTER OF Latin conquerors and indigenous populations in the areas bordering the eastern Mediterranean has on the whole not yet been properly explored. Even so, it is quite possible to identify some distinctive features of the relationship between the two communities and to describe their respective evolution. In all these areas, just as in Frankish Morea, religious affiliation provided the basic criterion of social stratification. The Latins or Franks constituted the society of the rulers. Whether one was Muslim or belonged to one of the numerous Oriental churches, a local inhabitant and his descendants were relegated to a lower status. Beyond this, one might have expected that nonfeudal groups of conquerors, who would not be bound by the social prejudice that stems from a strong class-consciousness, would be more accommodating toward the ruled. Evidence, however, points to the contrary. Exclusiveness is the dominant feature of the attitude adopted by the conquerors in Venetian Crete, Catalan Athens, and Genoese Chios. Local inhabitants were strictly excluded from joining the ruling elite and prevented from infiltrating into its ranks or encroaching upon its positions. Even adoption of the Roman creed could not alter their social status.

In Venetian Crete, where intermarriage between Latins and Greeks was prohibited and strictly enforced during the thirteenth and fourteenth centuries, Greeks were also barred from holding military tenures provided by the Venetian government and, as a result, from becoming members of the Cretan *Maggior Consiglio*, the most important local assembly participating in the government of the island. Only a small number of families of Greek *archontes* were granted special privilege to hold military tenures, and fewer still could wed Latin women. Besides, Greeks had no access to important administrative posts and could not serve as

<sup>142</sup> *French chronicle*, pars. 802–16. For the site of this castle, see Bon, *La Morée*, 381, 513.

<sup>143</sup> The exact date cannot be ascertained. See Jacoby, "Quelques considérations," 142 n. 37, and Bon, *La Morée*, 202.

mercenaries, while Latins who had married Greek women were to be dismissed. In practice the local Venetian authorities did not always hold to these rules and occasionally had to be reminded of them by the central government, but the official Venetian policy was clear, and, as far as the *archontes* were concerned, it was indeed implemented. As I have already suggested, this situation accounts for the role played by the latter as leaders of the Greek opposition to Venetian rule.<sup>144</sup>

In Catalan Greece the marriage of Latin women to Greeks was forbidden, a restriction that remained valid even in the rare cases when the latter were granted the status enjoyed by Franks or conquerors.<sup>145</sup> Catalan legislation was more lenient than was Venetian legislation in Crete, as it did not totally prohibit intermarriage, and Latins could marry Greek women; but it nevertheless prevented Greeks from integrating into the Latin elite. In Chios members of the Greek upper class were ousted from the citadel of the city shortly after its conquest in 1346 by the Genoese, and the descendants of those who participated in the conquest combined to form an association, the *albergo* of the Giustiniani, to which only few Greeks gained access.<sup>146</sup>

The special significance of these three cases is that the ruling elite did not belong to a feudal nobility. Unlike the feudalized areas, no link existed here between occupation and social status. The conquerors and those who joined them dwelt in urban centers, together with the indigenous Greek population, and pursued various economic activities similar to those of their neighbors. Everyday life brought them into close contact with the ruled, and hardly any factor save religion differentiated the two groups. Constant social and economic intercourse exerted a corrosive influence upon the social supremacy and political prerogatives acquired by conquest and permanently threatened them. As the danger of assimilation to the local Greek society, which was numerically far superior, was clearly perceived by the conquerors at the outset, they resorted to such measures of self-defense as total or partial prohibition of intermarriage to create an efficient barrier between themselves and the ruled, especially the upper class. Segregation was the most suitable means to preserve their separate group identity.

By contrast, the areas into which feudalism was transplanted underwent a different social development. In the Latin Kingdom of Jerusalem the feudal conquerors did not face any problem of integration, as the members of the local elite were exterminated or fled the cities captured by the Crusaders at the time of the conquest. Exclusiveness and class-

<sup>144</sup> For the time being, see Thiriet, *La Romanie vénitienne*, 131-32, 134-35, 291-93.

<sup>145</sup> As attested by a privilege bestowed upon a Greek inhabitant of Livadia, presumably in 1311; for the dating of this document, see David Jacoby, "La 'Compagnie catalane' et l'état catalan de Grèce. Quelques aspects de leur histoire," *Journal des Savants* (Apr.-June 1966), 88-89.

<sup>146</sup> See Philip P. Argenti, *The Occupation of Chios by the Genoese and their Administration of the Island, 1346-1566* (Cambridge, 1958), 1: 332-33, 582-612.



consciousness as displayed by the knights were encouraged by the rapid success of the conquest and the encounter with a basically agrarian population, whose occupation was considered an attribute of lowly social status.<sup>147</sup> Almost since its conquest by the Crusaders in 1191, Cyprus was closely connected with the Kingdom of Jerusalem, from which there came both the special brand of feudalism introduced into the island and many of the knights who settled it. In Cyprus as in Frankish Morea the knightly class confronted an existing Greek elite, although conditions in Cyprus were different from those in the Peloponnesus in the early thirteenth century. The conquest of the island was rapid, and two revolts were ruthlessly crushed thereafter, within less than a year. These events account for the uncompromising policy adopted toward the indigenous population. Besides, one may safely assume that the attitude and concepts of the Cypriot knightly class were molded to a large extent by those that had previously evolved in the Kingdom of Jerusalem. Integration of local notables was therefore excluded, the few exceptions encountered in the fourteenth century merely confirming the rule.<sup>148</sup>

Of all the areas mentioned above, Norman Italy is certainly the one whose social evolution most closely resembles that of the Morea. Descendants of the Greek and Lombard notables were fully integrated into the knightly class and held fiefs, both feudal and patrimonial land, or only the latter. In this last case, they were considered "knights who hold nothing" (*milites qui nihil tenent*), an expression that implies a tendency to assimilate these notables and their patrimonial estates respectively to the Norman conquerors and their fiefs, which carried military service. The survival of Byzantine and Lombard law in Norman Italy and its application to patrimonial estates offers yet another parallel to Frankish Morea,<sup>149</sup> although the analogous social evolution should not be ascribed to the influence of the Angevins, heirs of the Norman kingdom of Sicily, who also ruled Frankish Morea since 1278. It is at least partially attributable to the conditions of the conquest, which was slow and progressive in both areas and which, therefore, induced the conquerors to adopt a realistic and conciliatory attitude toward the indigenous population, especially the local upper class.

As is evident from this brief discussion, many complex problems arose from the encounter of Western conquerors and local society in the eastern Mediterranean, problems that suggest the benefits to be gained from

<sup>147</sup> The latest study on the subject is by Joshua Prawer, *The Crusaders' Kingdom. European Colonialism in the Middle Ages* (New York, 1972), especially 46-76, 504-33. The author does not distinguish, however, between the attitude of the knights and that of the burgesses toward the local population. His assertion (on p. 68) that knights married local women is not supported by any evidence, and the *pullani*, or *poulains*, of mixed stock were obviously children of Frankish burgesses and native mothers.

<sup>148</sup> See Louis de Mas Latrie, *Histoire de l'île de Chypre sous le règne de la maison de Lusignan* (Paris, 1852-61), 1: 135-40; George Hill, *A History of Cyprus* (Cambridge, 1948), 2: 8.

<sup>149</sup> See Claude Cahen, *Le régime féodal de l'Italie normande* (Paris, 1940), 53-54, 124-27.

intensive comparative study. The social, legal, and institutional evolution generated by this encounter can be fairly well reconstructed. But, beyond this evolution, such a study might enable us to discover the mental patterns underlying the attitudes and behavior of individuals and, especially, of social classes and societies reacting to each other in conquered areas.

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## The New Deal, National Socialism, and the Great Depression

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THE GREAT DEPRESSION of the 1930s was a unique phenomenon in that it happened simultaneously over almost the entire globe. It was experienced directly, not merely through its repercussions, by the people of nearly every nation and social class. Neither of the so-called world wars of this century was so pervasive, and while many distinct combinations of past events, such as the French Revolution, may be said to have had global results, these usually have been felt only over extended periods of time, long after the "event" itself has ended. The depression therefore presents a remarkable opportunity for historians interested in comparative study and analysis. It provides a kind of independent variable; when we look at how different nations or groups of people responded to the Great Depression, we can be sure, at least in a sense, that we are examining one single "thing," the existence of which was universally recognized at the time. Contemporaries disagreed among themselves about the causes of the depression (to say nothing of their disagreements about how it might be ended), but that there *was* a world-wide depression and that their own depression was related directly to those of their fellows, few denied.<sup>1</sup>

In this article I shall compare the response to the depression in the United States and Germany during the period from 1933 to about 1936 or 1937—that is, during the early years of the regimes of Franklin Roosevelt and Adolf Hitler. The choice is neither capricious nor perverse. I hope to

Earlier versions of this paper were read at the University of Cologne on May 15, 1972, and at the meeting of the Organization of American Historians in Chicago on April 13, 1973.

<sup>1</sup> See, for example, League of Nations, *The Course and Phases of the World Economic Depression* (Geneva, 1931) and the annual volumes of the league's *World Economic Survey* (Geneva, 1931–39); Paul Einzig, *The World Economic Crisis: 1929–1931* (London, 1931); Lionel Robbins, *The Great Depression* (London, 1934); Eugen Varga, *The Great Crisis and Its Political Consequences* (New York, 1934); Richard Lewisohn, *Histoire de la crise* (Paris, 1934); Bertrand Nogaro, *La crise économique dans le monde et en France* (Paris, 1936); Adolf Sturmthal, *Die grosse Krise* (Zurich, 1937); H. V. Hodson, *Slump and Recovery: 1929–1937* (Oxford, 1938); Jean Lescure, *Des crises générales et périodiques de surproduction* (5th ed.; Paris, 1938); Joseph A. Schumpeter, *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process* (New York, 1939). There is also an enormous contemporary literature on the depression in individual nations, a common theme of which is the universality of the phenomenon.

demonstrate that Nazi and New Deal antidepression policies displayed striking similarities. Since the two systems, seen in their totality, were fundamentally different, these similarities tell us a great deal about the depression and the way people reacted to it.

The differences between nazism and the New Deal scarcely need enumeration; within the context of Western industrial society two more antithetical systems would be hard to imagine. The Nazis destroyed democratic institutions. They imprisoned and murdered dissidents, even those, such as the Jews, who simply did not fit their image of a proper German. The New Dealers, whatever their limitations, threw no one in jail for his political beliefs and actually widened the influence of underprivileged elements in the society. Furthermore the historical experience, the traditions, and the social structure of the two nations could hardly have been more unlike. The Great War and its aftermath affected them in almost diametrically opposite ways. All the major economic groups in the two countries—farmers, industrialists, factory workers, and so on—confronted the problems of the depression with sets of expectations and values that differed greatly.

But these were the industrial nations most profoundly affected by the Great Depression, measured by such criteria as the percentage decline of output, or by the degree of unemployment. When Hitler and Roosevelt came to power both nations were in desperate straits; Hitler and Roosevelt followed leaders who had spectacularly failed to inspire public confidence in their policies. Both the severity of the depression and the sense of despair and crisis that existed in Germany and America in early 1933 set the stage for what followed.

I have focused on the early New Deal and Nazi years because at that time the new governments were primarily concerned with economic problems resulting from the depression. Hitler's expansionist ambitions no doubt existed from the beginning, but it was not until after the adoption of the Four Year plan in 1936 that he turned the German economy toward large-scale preparation for war.<sup>2</sup> Similarly, although his motive was clearly defensive, after 1937 Roosevelt also began to be influenced by military considerations.

Needless to say, by considering the similarities in American and German experiences during the depression, I do not mean to suggest that the New Deal was a form of fascism or still less that nazism was anything but an unmitigated disaster. I slight the basic differences between the New Deal and Nazi experiments here partly because they are well known but also because the differences did not affect economic policy as much as might be expected. The worse horrors of nazism were unrelated

<sup>2</sup> David Schoenbaum, *Hitler's Social Revolution: Class and Status in Nazi Germany, 1933-1939* (New York, 1967), 113; Dieter Petzina, *Autarkiepolitik im Dritten Reich: Der national-sozialistische Vierjahresplan* (Stuttgart, 1968), 26-39. Cf. Gerhard Meink, *Hitler und die deutsche Aufrüstung: 1933-1937* (Wiesbaden, 1959).

to Nazi efforts to overcome the depression. Hitler's destruction of German democracy and his ruthless persecution of Jews had little impact on the economy as a whole. Discharging a Jew and giving his job to an "Aryan" did not reduce unemployment. The seizure of Jewish property merely transferred wealth within the country; it did not create new wealth. Moreover, actions undertaken by New Dealers and Nazis for different reasons often produced similar results. My argument concentrates on policies and their effects, not on the motives of the policy makers.

Finally, the fact that countless Germans were deluded by Nazi rhetoric (or that large but lesser numbers were repelled by the system) does not mean that nothing the Nazis did helped anyone but themselves and their sympathizers. Moral abhorrence should no more blind us to the success of some Nazi policies than should admiration of the objectives of the New Deal to its failures. As the English economic historian C. W. Guillebaud warned in *The Social Policy of Nazi Germany*, written in the midst of the Battle of Britain, "Modern Germany is a highly complex phenomenon, with much that is good and bad in it, and nothing is achieved except distortion and absence of reality by any attempt to reduce it to a simple picture of a vast population deluded and oppressed by a small number of brutal gangsters."<sup>3</sup>

CONSIDER FIRST how the two governments dealt with poverty and mass unemployment. Both combined direct relief for the indigent with public-works programs to create jobs. The Americans stressed the former, the Germans the latter, with the result that while acute suffering was greatly reduced in both nations, unemployment declined much more rapidly in Germany. Congress appropriated \$3.3 billion for public works in 1933, but Roosevelt, unconvinced that public works would stimulate the economy and fearful of waste and corruption, did not push the program. Briefly, during the winter of 1933-34, he allowed Harry Hopkins to develop his Civil Works Administration, which found jobs for over four million people, but in the spring the program was closed down to save money. Only in 1935 did federal public works become important. Then, under Hopkins's Works Progress Administration and Harold L. Ickes's Public Works Administration, countless roads, schools, bridges, dams, and public buildings were constructed.<sup>4</sup> The Germans, on the other hand, immediately launched an all-out assault on unemployment. Expanding upon policies initiated under Franz von Papen and Kurt von Schleicher, they stimulated private industry through subsidies and tax rebates, encouraged consumer spending by such means as marriage loans, and plunged into the massive

<sup>3</sup> C. W. Guillebaud, *The Social Policy of Nazi Germany* (Cambridge, 1942), 132.

<sup>4</sup> Searle F. Charles, *Minister of Relief: Harry Hopkins and the Depression* (Syracuse, 1963), 44-52, 61, 65, 159-65, 230-31; William E. Leuchtenburg, *Franklin D. Roosevelt and the New Deal: 1932-1940* (New York, 1963), 121-30, 133.

public-works program that produced the autobahns, and housing, railroad, and navigation projects.<sup>5</sup> If some New Deal projects seemed to critics wasteful and unnecessary, so did the Nazi penchant for gigantic stadiums and other public buildings, as described in Albert Speer's memoirs. The American boondoggle had its parallel in what the Germans called *Pyramidenbau*, pyramid-building.<sup>6</sup>

It is fashionable, and not of course inaccurate, to note the military aspect of German public-works policies, although in fact relatively little was spent on rearmament before 1935.<sup>7</sup> It is less fashionable, but no less accurate, to point out that the aircraft carriers *Yorktown* and *Enterprise*, four cruisers, many lesser warships, as well as over one hundred army planes and some fifty military airports (including Scott Field in Illinois, the new Air Force headquarters) were built with Public Works Administration money—more than \$824 million of it.<sup>8</sup> There was, furthermore, little difference in appearance or intent between the Nazi work camps and those set up in America under the Civilian Conservation Corps. Unlike the public-works programs, these camps did not employ many industrial workers who had lost their jobs, nor were they expected to have much of a stimulating effect on private business. Both employed enrollees at forestry and similar projects to improve the countryside and were essentially designed to keep young men out of the labor market. Roosevelt described work camps as a means for getting youth “off the city street corners,” Hitler as a way of keeping them from “rotting helplessly in the streets.” In both countries much was made of the beneficial social results of mixing

<sup>5</sup> Dieter Petzina, “Hauptprobleme der deutschen Wirtschaftspolitik 1932/33,” *Vierteljahrshefte für Zeitgeschichte*, 15 (1967): 18–30, 43–50; C. W. Guillebaud, *The Economic Recovery of Germany: From 1933 to the Incorporation of Austria in March 1938* (London, 1939), 38–42, 51–52; René Erbe, *Die nationalsozialistische Wirtschaftspolitik, 1933–1939, im Lichte der modernen Theorie* (Zurich, 1958), 91–92, 182–86; Heinrich Bennecke, *Wirtschaftliche Depression und politischer Radikalismus: 1918–1938* (Munich, 1970), 300–01; Leo Grebler, “Work Creation Policy in Germany: 1932–1935,” *International Labour Review*, 35 (1937): 329–51, 505–27; Albert Speer, *Inside the Third Reich: Memoirs* (New York, 1971), 114–19, *passim*; Fritz Thyssen, *I Paid Hitler* (New York, 1941), 143–47.

<sup>6</sup> For a convenient tabular summary of both pre-Hitler and Nazi public-works activities, see Heinrich Dräger, *Arbeitsbeschaffung durch produktive Kreditschöpfung* (4th ed.; Düsseldorf, [n.d.]), 165. On the antecedents of the Nazi program, see Gerhard Kroll, *Von der Weltwirtschaftskrise zur Staatskonjunktur* (Berlin, 1958), 421–55, 458, 461–63.

<sup>7</sup> Burton J. Klein, *Germany's Economic Preparation for War* (Cambridge, Mass., 1959), 16–17, 254; Hjalmar Schacht, 1933: *Wie eine Demokratie stirbt* (Düsseldorf, 1968), 86–91. Through 1934, Nazi expenditure for armaments totaled 4.9 million marks. Wolfram Fischer, *Deutsche Wirtschaftspolitik: 1819–1945* (Opladen, 1968), 102.

<sup>8</sup> Public Works Administration, *America Builds: The Record of PWA* (Washington, 1939), 36–37, 105–06, 290–91; Arthur M. Schlesinger, jr., *The Coming of the New Deal* (Boston, 1959), 287–88. The first New Deal authorization for public works was part of the National Industrial Recovery Act and provided for “the construction of naval vessels within the terms and/or limits of the London Naval Treaty of 1930 and of aircraft required therefor and construction of heavier-than-air aircraft and technical construction for the Army Air Corps and such Army housing projects as the President may approve, and provision of original equipment for the mechanization or motorization of such Army tactical units as he may designate.” Henry S. Commager, ed., *Documents of American History* (2d ed.; New York, 1941), 2: 455.



thousands of young people from different walks of life in the camps and of the generally enthusiastic response of youth to the camp experience.<sup>9</sup>

Furthermore, both were organized on semimilitary lines with the subsidiary purposes of improving the physical fitness of potential soldiers and stimulating public commitment to national service in the emergency. Putting the army in control of hundreds of thousands of young civilians roused considerable concern in the United States. This concern proved to be unfounded; indeed, the army undertook the task with great reluctance and performed it with admirable restraint. It is also difficult to imagine how so large a program could have been inaugurated in so short a time in any other way. The CCC program nevertheless served paramilitary and patriotic functions not essential to its announced purpose. Corpsmen were required to stand "in a position of alertness" while speaking to superiors and to address them as "Sir." Camp commanders possessed mild but distinctly military powers to discipline their men, including the right to issue dishonorable discharges. Morning and evening flag-raising ceremonies were held as "a mark," the civilian director of the CCC, Robert Fechner, explained, "of patriotism, of good citizenship and of appreciation by these young men of the thoughtful care being given them by their government." Army authorities soon concluded that six months' CCC service was worth a year's conventional military training, and Secretary of War George Dern claimed that running the camps provided the army with the best practical experience in handling men it had ever had. Summing up the military contribution of the CCC, John A. Salmond, the most sympathetic historian of the agency, wrote:

To a country engaged in a bloody war, it had provided the sinews of a military force. It had given young officers valuable training in command techniques, and the nearly three million young men who had passed through the camps had received experience of military life upon which the Army was well able to build.<sup>10</sup>

New Deal and Nazi attempts to stimulate industrial recovery also resembled each other in a number of ways. There was at the start much jockeying for position between small producers and large, between

<sup>9</sup> Roosevelt stressed "the moral and spiritual value" of camp life, Hitler its "class reconciling" function. Samuel I. Rosenman, ed., *The Public Papers and Addresses of Franklin D. Roosevelt* (New York, 1938), 2: 81; Max Domarus, ed., *Hitler: Reden und Proklamationen* (Munich, 1965), 1: 321.

<sup>10</sup> Guillebaud, *Economic Recovery of Germany*, 41, 201, 225-26; Schoenbaum, *Hitler's Social Revolution*, 63-64, 79; Erbe, *Wirtschaftspolitik*, 90; Domarus, *Reden und Proklamationen*, 1: 212; John A. Salmond, *The Civilian Conservation Corps, 1933-1942: A New Deal Case Study* (Durham, 1967), 221, *passim*; Charles W. Johnson, "The Army and the Civilian Conservation Corps, 1933-42," *Prologue*, 4 (1972): 139-56; Schlesinger, *Coming of the New Deal*, 337-40; Rosenman, *Public Papers*, 2: 82; Frances Perkins, *The Roosevelt I Knew* (New York, 1946), 177-80; William E. Leuchtenburg, "The New Deal and the Analogue of War," in John Braeman, Robert H. Bremner, and Everett Walters, eds., *Change and Continuity in Twentieth-Century America* (Columbus, 1964), 114-17.

manufacturers and merchants, between inflationists and deflationists, between planners, free enterprisers, and advocates of regulated competition. In Germany the great financiers and the leaders of the cartelized industries, most of them bitterly opposed to democratic institutions, demanded an authoritarian solution that would eliminate the influence of organized labor and increase their own control over the economy, whereas small operators, shopkeepers, and craftsmen wanted to reduce the power of bankers and to destroy not only the unions but also the industrial monopolies and chain stores. The former sought to manipulate the Nazis, the latter comprised, in the main, the Nazis' enthusiastic supporters, but Hitler and the party felt and responded to pressures from both camps.<sup>11</sup> In the United States most big business interests had no open quarrel with the existing order, but by 1933 many were calling for suspension of the antitrust laws in order to end the erosion of profits by competitive price cutting. Other interests wanted to strengthen the antitrust laws, still others favored various inflationary schemes, still others some attempt at national economic planning. All clamored for the attention of the new administration.<sup>12</sup>

The ideas of these groups were contradictory, and neither Roosevelt nor Hitler tried very hard to resolve the differences. Roosevelt's method was to suggest that the contestants lock themselves in a room until they could work out a compromise. But Hitler, who freely admitted to being an economic naïf, was no more forceful. "I had to let the Party experiment," he later recalled in discussing the evolution of his industrial recovery program. "I had to give the people something to do. They all wanted to help. . . . Well, let them have a crack at it."<sup>13</sup>

Out of the resulting confusion emerged two varieties of corporatism, a conservative, essentially archaic concept of social and economic organization that was supposed to steer a course between socialism and capitalist plutocracy. Corporatist theory argued that capitalists and workers (organized in industry-wide units) should join together to bring order and profit to each industry by eliminating competition and wasteful squabbling

<sup>11</sup> Schoenbaum, *Hitler's Social Revolution*, 115-24; Arthur Schweitzer, *Big Business in the Third Reich* (Bloomington, 1964), 4-5, 75-79; L. Hamburger, *How Nazi Germany Has Controlled Business* (Washington, 1943), 113-14; Raimund Rämisch, "Der berufsständische Gedanke als Episode in der nationalsozialistischen Politik," *Zeitschrift für Politik*, 4 (1957): 263-72. Schoenbaum describes the situation as "a real, if muffled, struggle, not about ideological principles but about the control and direction of the economy" (p. 122), and Schweitzer writes: "We must give up the notion of the [Nazi] state as a unified . . . entity developing an economic policy. . . . Instead we must see who stood behind the state and who originated each economic goal and supported the subsequent economic policy" (p. 4).

<sup>12</sup> Ellis W. Hawley, *The New Deal and the Problem of Monopoly: A Study in Economic Ambivalence* (Princeton, 1966), 21-25; Schlesinger, *Coming of the New Deal*, 179-85.

<sup>13</sup> Schlesinger, *Coming of the New Deal*, 98; Schoenbaum, *Hitler's Social Revolution*, 123-24. Hitler's intention to use economic policy to achieve his political objectives was clear from the start, Petzina writes, but the particular policies adopted in 1933 were "contradictory and heterogeneous." The idea that the Nazis had a thoroughly developed recovery program in 1933 became current only after the recovery had taken place. Petzina, "Hauptprobleme," 50.

between labor and management. These associations should be supervised by the government in order to protect the public against monopolistic exploitation. In 1933 corporatism was already being experimented with by the Portuguese dictator Antonio de Oliveira Salazar and more tentatively by Benito Mussolini. It also had roots in American and German experience. The American trade association movement of the 1920s reflected basic corporatist ideas (with the important exception that industrialists were opposed to government representation in their councils). When the depression undermined the capacity of these "voluntary" associations to force individual companies to honor the associations' decisions, some trade association leaders became willing to accept government policing as a necessary evil. Among others, Gerard Swope of the General Electric Company attracted considerable attention in 1931 with his Swope Plan for a nationwide network of compulsory trade associations supervised by the Federal Trade Commission. President Herbert Hoover, who had been among the most ardent supporters of trade associations, denounced the Swope Plan as both a threat to industrial efficiency and "the most gigantic proposal of monopoly ever made." He considered all such compulsory schemes fascistic. But a number of early New Dealers—Hugh Johnson, Donald Richberg, and Lewis Douglas among others—found corporatism appealing. In Germany the concept of government-sponsored cartels that regulated output and prices had a long tradition, but the existence of powerful trade unions precluded the possibility of a truly corporative organization before 1933. Hitler's success changed that swiftly. Nazi ideologues such as Gottfried Feder combined with big industrialists like Fritz Thyssen and leaders of small business interests like Dr. Heinrich Meusch to push the corporative approach. The works of one of the leading theorists of corporatism, Professor Othmar Spann of the University of Vienna, were widely discussed in Germany in 1933, and the Nazis established a complex system of "estates" governing all branches of industry.<sup>14</sup>

In America the process went not nearly so far, but the system of self-governing industrial codes established under the National Recovery Administration was obviously in the same pattern.<sup>15</sup> Production controls,

<sup>14</sup> Herman Lebovics, *Social Conservatism and the Middle Classes in Germany: 1914-1933* (Princeton, 1969), 109-10, 133-34; Taylor Cole, "Corporative Organization in the Third Reich," *Review of Politics*, 2 (1940): 438-62; Thyssen, *I Paid Hitler*, 122-27; Schweitzer, *Big Business*, 79-81, 135-36, 248-49; J. George Frederick, ed., *The Swope Plan: Details, Criticisms, Analysis* (New York, 1931), 19-45; W. S. Myers and W. H. Newton, *The Hoover Administration: A Documented Narrative* (New York, 1936), 119, 155; Herbert Hoover, *The Memoirs of Herbert Hoover: The Great Depression, 1929-1941* (New York, 1952), 420; Joseph Dorfman, *The Economic Mind in American Civilization: 1918-1933* (New York, 1959), 4: 48-50; 5: 632-34; Hawley, *New Deal and Monopoly*, 42. See also, Matthew H. Elbow, *French Corporative Theory, 1789-1948: A Chapter in the History of Ideas* (New York, 1953), 122-67; and Roland Sarti, *Fascism and the Industrial Leadership in Italy, 1919-1940: A Study in the Expansion of Private Power under Fascism* (Berkeley, 1971), 69-104.

<sup>15</sup> When a reporter asked Roosevelt in November 1933, for his opinion of an updated version of the Swope Plan, he replied: "Mr. Swope's plan is a very interesting theoretical suggestion in regard to some ultimate development of N.R.A." Rosenman, *Public Papers*,

limitation of entry, and price and wage manipulation were common characteristics of government policy in both countries. So were the two governments' justifications of drastic and possibly illegal or unconstitutional<sup>16</sup> changes in the way the economy functioned on the ground that a "national emergency" existed, and the enormous propaganda campaigns they mounted to win public support.

The drafters of the National Industrial Recovery Act were not deliberately imitating fascist corporatism (although Hugh Johnson, a key figure among them, was an admirer of Mussolini). *Fortune*, which devoted an entire issue in 1934 to an analysis of the Italian system, was scarcely exaggerating when it stated that corporatism was "probably less well known in America than the geography of Tibet." As Gilbert H. Montague, a lawyer who had played a small role in the design of the code system, later wrote, the NRA was only "unconsciously" fascistic. It would be hard to find a better illustration of the common impact of the depression on two industrial nations committed to the preservation of capitalism.<sup>17</sup>

During the early stages big business interests dominated the new organizations and succeeded in imposing their views on government. In Germany the radical Nazi artisan socialists who wanted to smash the cartels and nationalize the banks, led by Gregor Strasser and Gottfried Feder, lost out to the powerful bankers and industrialists, represented by Hjalmar Schacht. In the United States victory went to the large corporations in each industry, which dominated the new code authorities.

But bewildering crosscurrents of interest and faction hampered the functioning of corporatism. In theory the system promised harmony and efficiency within industries, but in practice it seldom provided either. It did not even pretend to solve interindustry conflicts, yet these were often more disturbing to government authorities. Under corporatism workers were supposed to share fairly in decision making and in the rewards resulting from the elimination of conflict and competition; in both countries industrialists resisted allowing them to do so, with the con-

2: 447. Hoover also noted the connection between the Swope Plan and NRA. He entitled the chapter on the NRA in his memoirs, "Fascism Comes to Business." Hoover, *Great Depression*, 420-29.

<sup>16</sup> Although many Supreme Court decisions declaring New Deal laws unconstitutional reflected the unreasonably narrow views of five conservative justices, much of the early "emergency" legislation was very loosely worded. Furthermore the decision invalidating the NIRA (*Schechter Poultry Corp. v. United States*, 295 U.S. 495) was a unanimous one. On Roosevelt's unwillingness to abide by conventional constitutional limitations on federal power, see Arthur M. Schlesinger, jr., *The Politics of Upheaval* (Boston, 1960), 258, 452-53, and Paul L. Murphy, *The Constitution in Crisis Times: 1918-1969* (New York, 1972), 128-38, 246-47. Roosevelt "was under no illusion about the constitutional status of . . . his program," Schlesinger writes (p. 260), and Murphy calls him "a constitutional opportunist and pragmatist" (p. 246).

<sup>17</sup> Hawley, *New Deal and Monopoly*, 53-55, 66; Schlesinger, *Coming of the New Deal*, 96, 112-16, 153-54; *Fortune*, 10 (1934): 45; Gilbert H. Montague, "Is NRA Fascistic?" in American Academy of Political and Social Science, *Annals*, 180 (1935): 159.

sequence that the governments found themselves being pushed to enforce compliance. In America workingmen were a potent political force and a vital element in the New Deal coalition. German workers did not count as voters after 1933, but their cooperation and support remained essential to Nazi ambitions. Small businessmen also maintained a steady drum-fire of complaint, and both New Dealers and Nazis were sensitive to their pressure. Even the great industrialists were sometimes at odds with the system. Many German tycoons objected to sharing authority with labor and small producers, others to particular decisions imposed on the new estates by the government. German steel and chemical manufacturers like the Krupps and the I. G. Farben interests benefited from Hitler's emphasis on building up war-oriented industry and backed him enthusiastically, but producers dependent upon foreign raw materials or primarily concerned with the manufacture of consumer goods suffered from Nazi trade and monetary policies and held back. And as for the American industrialists, however much they profited under NRA codes, most of them came increasingly to resent the regimentation that codes entailed and to fear the growing interference in their affairs by bureaucrats.<sup>18</sup>

To the Nazis corporatism seemed at first compatible with the political process called *Gleichschaltung*, or coordination, a process by which nearly every aspect of life in their totalitarian state was brought under the control of Hitler and the party. It quickly became apparent, however, that the autonomous character of any corporatist organization made direct control from above difficult. America, fortunately, was never *gleichgeschaltet*. But, in any case, by 1935 and 1936 the Roosevelt and Hitler governments were abandoning corporatism and taking a more anti-big-business stance. In America this meant, aside from the demise of the NRA, more support for industrial labor, stricter regulation of public utilities, higher taxes on the rich and on corporations, rhetorical attacks on "economic royalists," and—by 1938—revived enforcement of the antitrust laws. In Germany, although the traditional cartel structure was retained, it involved limitations on corporate dividends; forced reductions in the interest rates paid on government bonds; government construction and operation of steel, automobile, and certain other facilities in competition with private enterprise; and higher taxes on private incomes and on corporate profits. As in the United States, but to a much greater degree, freedom of managerial decision making was sharply curtailed.<sup>19</sup>

<sup>18</sup> Schweitzer, *Big Business*, 114–30, 142–46, 156–76, 538–47; Schoenbaum, *Hitler's Social Revolution*, 115, 123; Cole, "Corporative Organization," 449–50; Lebovics, *Social Conservatism*, 134–35; Hawley, *New Deal and Monopoly*, 53–93; Irving Bernstein, *Turbulent Years: A History of the American Worker, 1933–1941* (Berkeley, 1969), 38–41, 217–18.

<sup>19</sup> Hawley, *New Deal and Monopoly*, 153–55, 301, 346–57, 420–55; Sidney Ratner, *Taxation and Democracy in America* (New York, 1967), 470–78; Irving Bernstein, *The New Deal Collective Bargaining Policy* (Berkeley, 1950), 112–28; J. Joseph Huthmacher, *Senator Robert F. Wagner and the Rise of Urban Liberalism* (New York, 1968), 189–98; Erbe, *Wirtschaftspolitik*,

The success or failure of American and German efforts to stimulate industrial recovery is a separate question not central to my argument here. What is central to the argument is this: both were marked by vacillation, confusion, and contradictions, by infighting within the administering bureaucracies, by an absence of any consistently held theory about either the causes of the depression or how to end it. Both also subordinated economic to political goals. The "primacy of politics" in Nazi Germany is a commonplace, its most glaring expression occurring in 1936 when shortages of raw materials and foreign exchange led Hitler to choose between guns and butter. He chose, of course, guns. The problem could be solved by an act of will, he insisted; it was the task of the economy to supply the military needs of the state—so be it! When Schacht, his chief economic adviser, urged a more balanced use of available resources, the Führer fired him. Such ruthless subordination of economic interests to the state did not occur in the United States, although when military considerations began to dominate American policy after 1939 Roosevelt was also prepared to substitute guns for butter. I need only mention his famous announcement that he was replacing "Dr. New Deal" with "Dr. Win the War" as his prime consultant.<sup>20</sup>

But conventional "politics"—the accommodation of political leaders to the pressures of interest groups—affected economic policy in both nations. Beset by business interests seeking aid, by trust busters eager to break up the corporate giants, by planners brimming with schemes to rationalize the economy, the Roosevelt administration survived in a state of constant flux, making concessions to all views, acting in contradictory and at times self-defeating ways. "The New Dealers," writes Ellis W. Hawley, "failed to arrive at any real consensus about the origins and nature of economic concentration." Nor did they follow any consistent policy in the fight against the industrial depression. And Roosevelt's inconsistency, as Hawley also notes, "was the safest method of retaining political power, . . . a political asset rather than a liability." The Nazis, as I have shown, also permitted pressures from various economic interests to influence policy. They did so partly because even a totalitarian dictatorship could profit from the active cooperation of powerful economic groups and partly because the Nazi party had no fixed economic beliefs. Roosevelt responded to pressure groups, Hitler for a time suffered them to exist—a most vital distinction—but the practical result was the same. Put differently, Hitler

83-87, 143-44, 181-82; Klein, *Germany's Economic Preparation for War*, 42-44; Enno Georg, *Die wirtschaftlichen Unternehmungen der SS* (Stuttgart, 1963), 42-58; Hamburger, *How Nazi Germany Controlled Business*, 67-70; Rämisch, "Berufsständische Gedanken," 270-72. The effects of fascist policy on the power of business leaders are brilliantly described in Ernst Basch [E. B. Ashton] *The Fascist: His State and His Mind* (New York, 1937), 110-16.

<sup>20</sup> T. W. Mason, "The Primacy of Politics—Politics and Economics in National Socialist Germany," in S. J. Woolf, ed., *The Nature of Fascism* (New York, 1968), 165-95; Petzina, *Autarkiepolitik*, 35-36, 49; Wilhelm Grottkopf, *Die grosse Krise: Lehren aus der Überwindung der Wirtschaftskrise, 1929/32* (Dusseldorf, 1954), 299-300.



had a clear political objective—it was actually an obsession—but he was almost as flexible about specific economic policies as Roosevelt. “As regards economic questions,” he boasted in 1936, “our theory is very simple. We have no theory at all.”<sup>21</sup>

New Deal and Nazi labor policies were also shaped by the Great Depression in related ways. On the surface this statement may appear not simply incorrect but perverse, but only because of our tendency to identify labor with unionization. It is true that Hitler totally destroyed the German unions and that Roosevelt, in part unwittingly and surely with some reluctance, enabled American unions to increase their membership and influence enormously. But New Deal and Nazi policies toward unions had little to do directly with the depression and throw little light on the national policies toward workingmen. Hitler would no doubt have destroyed the Weimar unions as autonomous organizations in any case—he destroyed all autonomous organizations in Germany. But it was because they were anti-Nazi that he smashed the unions so quickly. Roosevelt was at first indifferent to organized labor; he encouraged the American unions in order to gain labor’s support, not to speed economic recovery. In each instance the decision was essentially political.<sup>22</sup>

It is not difficult to demonstrate Nazi concern for industrial workers. The “battle against unemployment” had first priority in 1933, and it was won remarkably swiftly: by 1936 something approaching full employment existed in Germany and soon thereafter an acute shortage of labor developed. Of course the military draft siphoned thousands of men out of the German labor market, contributing to the shortage, but this was also true in the United States after 1940. Certainly full employment was never approached in America until the economy was shifted to all-out war production.<sup>23</sup>

Moreover, Nazi ideology (and Hitler’s prejudices) inclined the regime to favor the ordinary German over any elite group. Workers—as distinct from “Marxist” members of unions—had an honored place in the system. To the extent that the Nazis imposed restrictions on labor, they did so for

<sup>21</sup> Hawley, *New Deal and Monopoly*, 470, 476; Mason, “Primacy of Politics,” 173–74; Cole, “Corporative Organization,” 449; Daniel R. Fusfeld, *The Economic Thought of Franklin D. Roosevelt and the Origins of the New Deal* (New York, 1956), 254. This does not mean, of course, that the ideas of American and German economists were not taken up by the politicians. Roosevelt was influenced by dozens of them; he did not, however, adopt any consistent line of economic reasoning. Both the Nazi program to create work and their post-1936 autarchic policies were apparently anticipated in Robert Friedlaender-Precht’s *Die Wirtschaftswende* (Leipzig, 1931), although Friedlaender-Precht, whose father was Jewish, was unable to publish or exert any direct influence on economic policy after 1933. See Grottkopf, *Die grosse Krise*, 35–36, 262; Kroll, *Weltwirtschaftskrise*, 435–55.

<sup>22</sup> Schoenbaum, *Hitler’s Social Revolution*, 74–75; Bernstein, *Turbulent Years*, 699, 778–79; Bernstein, *New Deal Collective Bargaining*, 82, 121–22, 127–28, 130–31.

<sup>23</sup> American unemployment never fell much below 8 million during the New Deal. In 1939 about 9.4 million were out of work, and at the time of the 1940 census (in March) unemployment stood at 7.8 million, almost fifteen per cent of the work force. *Historical Statistics of the United States, Colonial Times to 1957* (Washington, 1960), 73; Broadus Mitchell, *Depression Decade: From New Era through New Deal, 1929–1941*, (New York, 1947), 95.

the benefit of the state, not of employers. In a sense the Nazi Courts of Social Honor may even be compared with the New Deal National Labor Relations Board. These courts did not alter power relationships between capital and labor as the NLRB did; they represented the interests of the Nazi party rather than those of labor. But they did adjudicate disputes between workers and bosses, and there is considerable evidence that the Courts of Social Honor tended more often than not to favor workingmen in these disputes. Furthermore the very existence of these courts put considerable psychological pressure on employers to treat labor well.<sup>24</sup>

It is beyond argument that the Nazis encouraged working-class social and economic mobility. They made entry into the skilled trades easier by reducing the educational requirements for many jobs and by expanding vocational training. They offered large rewards and further advancement to efficient workers, and, in the Strength Through Joy movement, they provided extensive fringe benefits, such as subsidized housing, low-cost excursions, sports programs, and more pleasant factory facilities. Eventually the Nazi stress on preparation for war meant harder work, a decline in both the quantity and quality of consumer goods, and the loss of freedom of movement for German workers, but the hierarchy imposed these restrictions and hardships belatedly and very reluctantly because of its desire to win and hold the loyalty of labor. If the question is: "Did the Nazi system give workers more power?" the answer of course is that it did not. But that question, albeit important, has little to do with the actual economic position of workingmen or with the effectiveness of the Nazi system in ending the depression.<sup>25</sup>

NEW DEAL AND NAZI methods of dealing with the agricultural depression also had much in common. Both sought to organize commercial agriculture in order to increase farm income, under the New Deal Agricultural Adjustment Act through supposedly democratic county committees to control production, in Germany through the centralized Estate for Agriculture. The purpose was to raise agricultural prices and thus farm income through

<sup>24</sup> Schoenbaum, *Hitler's Social Revolution*, 73-90; Basch [Ashton], *The Fascist*, 92, 94; Erbe, *Wirtschaftspolitik*, 88-95; T. W. Mason, "Labour in the Third Reich," *Past and Present*, no. 33 (1966): 112-41; Guillebaud, *Economic Recovery of Germany*, 187-95; Karl D. Bracher, *The German Dictatorship: The Origins, Structure, and Effects of National Socialism* (New York, 1970), 332-35, 339.

<sup>25</sup> Schoenbaum, *Hitler's Social Revolution*, 95-112; Guillebaud, *Economic Recovery of Germany*, 196-212; Hans-Gerd Schumann, *Nationalsozialismus und Gewerkschaftsbewegung: Die Vernichtung der deutschen Gewerkschaften und der Aufbau der "Deutschen Arbeitsfront"* (Hanover, 1958), 132-34, 138-41, 144-45; Herbert Steinwarz, "The Amenities of Industry and Labour in Germany," *International Labour Review*, 36 (1937): 772-79; K. Mandelbaum, "An Experiment in Full Employment: Controls in the German Economy, 1933-1938," in Oxford University Institute of Statistics, *The Economics of Full Employment* (New York, 1967), 181, 194-201.

a system of subsidies, paid for in each instance by processing taxes that fell ultimately on consumers. Both governments also made agricultural credit cheaper and more readily available and protected farmers against loss of their land through foreclosures.

These similarities are not remarkable; nearly every nation sought, more or less in these ways, to bolster agricultural prices and protect its farmers. What is interesting, given the profound differences between American and German agriculture, is the attitudes of the two governments toward the place of farmers in the society and toward rural life. Although there was no American counterpart to Hitler's racist, anti-intellectual glorification of the German peasantry, Nazi thinking was at least superficially similar to that of generations of American farm radicals. (David Schoenbaum has aptly called Gottfried Feder "a kind of Central European William Jennings Bryan.") The typical American farmer was no more like a German peasant than the owner of a Southern plantation was like a Junker, but under the impact of the depression farmers large and small in both countries were expressing the same resentments and demands, and these affected Nazi and New Deal policies in related ways. Furthermore, the ideas of Roosevelt and Hitler about farmers were quite alike. Both tended to romanticize rural life and the virtues of an agricultural existence. They hoped to check the trend of population movement to the cities and to disperse urban-centered industries. Roosevelt spoke feelingly of the value of close contact with nature and of the "restful privilege of getting away from pavements and from noise." Only in the country, he believed, did a family have a decent chance "to establish a real home in the traditional American sense." He did not deny the attractions of city life, but he argued that electricity, the automobile, and other modern conveniences made it possible for rural people to enjoy these attractions without abandoning the farm. While governor of New York he set up a program for subsidizing unemployed city families on farms so that "they may secure through the good earth the permanent jobs they have lost in over-crowded cities and towns."<sup>26</sup>

Hitler called the German peasantry "the foundation and life source" of the state, "the counterbalance to communist madness," and "the source of national fertility." The superiority of rural over urban life was a Nazi dogma—especially the life of the self-sufficient small farmer, free from the dependency and corruption of a market economy. "The fact that a people is in a position to nourish itself from its own land and through that to lead its own life independent of foreign nations has always in

<sup>26</sup> Frank Freidel, *Franklin D. Roosevelt: The Triumph* (Boston, 1956), 224–26; Fusfield, *Economic Thought of FDR*, 84, 124–30; Franklin D. Roosevelt, "Back to the Land," *Review of Reviews*, 84 (1931): 63–64; State of New York, *Public Papers of Governor Franklin D. Roosevelt: 1931* (Albany, 1937), 752–59, 781–82. "Is it worthwhile," Roosevelt asked in a radio address late in 1931, "for us to make a definite effort to get people in large numbers to move out of cities . . . ? It seems to me that to that question we must answer an emphatic YES." *Ibid.*, 782.

history been significant," a Nazi agricultural expert wrote in 1935. "Families on the land also have the biological strength to maintain themselves and to compensate for population losses resulting from migration to cities and from war." Nazi leaders referred to Berlin as "Moloch Berlin" and deplored the influx of Germans from the east into the capital. The Nazis' housing policy sought to stimulate suburban development in order to bring industrial workers closer to the land and to reduce urban crowding. They placed all construction under government control, made funds available for low-interest, state-guaranteed mortgage loans, and provided tax relief to builders of small apartments and private homes.<sup>27</sup>

The Tennessee Valley Authority and the rural electrification program made important progress toward improving farm life, but efforts to reverse the population trend yielded very limited results. As president, Roosevelt dreamed of decentralizing industry and of relocating a million families on small farms, but during the whole of the New Deal his Resettlement Administration placed fewer than 11,000 families on the land; even the best-known of the settlements, Arthurdale in West Virginia, which benefited from the particular interest and financial support of Eleanor Roosevelt, never became a viable community until the outbreak of the war. That the Resettlement Administration was run by Rexford Tugwell, who considered the back-to-the-land movement impracticable, contributed to the ineffectiveness of this program, but the agency's greenbelt town program of planned suburban development, which Tugwell did think practicable, also produced miniscule results—only three of the sixty originally planned greenbelt towns were built.<sup>28</sup>

Although Nazi ideologues hoped to reverse Germany's urban-rural ratio, which was seventy per cent urban in 1933, their rural resettlement and "rurban" programs proved equally disappointing. Between 1933 and 1938 the Nazis resettled about 20,000 families, but this was scarcely more than half the number the Weimar government had managed to relocate between 1927 and 1932. Nor did the German "rurban" development program ever get very far off the ground. In both the American and German cases efforts to check the movement of population to the cities foundered

<sup>27</sup> Domarus, *Reden und Proklamationen*, 1: 174, 234-35, 304; Schoenbaum, *Hitler's Social Revolution*, 153-54; Martin Busse, "Bauer und Boden," in Otto Mönckmeier, ed., *Jahrbuch für nationalsozialistische Wirtschaft* (Stuttgart, 1935), 65-66; Johann W. Ludowici, *Das deutsche Siedlungswerk* (Heidelberg, 1935), 15; Walter Fey, "Leistungen und Aufgaben im Deutschen Wohnungs- und Siedlungsbau," *Institut für Konjunkturforschung*, special vol. 42 (Berlin, 1936), 24-29.

<sup>28</sup> Sidney Baldwin, *Poverty and Politics: The Rise and Decline of the Farm Security Administration* (Chapel Hill, 1968), 74-75, 88, 104-08; Joseph L. Arnold, *The New Deal in the Suburbs: A History of the Greenbelt Town Program, 1935-1954* (Columbus, 1971), 28-50, *passim*; Paul K. Conkin, *Tomorrow a New World: The New Deal Community Program* (Ithaca, 1959), 164-67, 237-55, 305-31; Joseph P. Lash, *Eleanor and Franklin: The Story of Their Relationship, Based on Eleanor Roosevelt's Private Papers* (New York, 1971), 393-417; Edward S. Shapiro, "Decentralist Intellectuals and the New Deal," *Journal of American History*, 58 (1972): 948-49. In a speech in April 1934, Roosevelt called the resettlement program "one of my pet children." Rosenman, *Public Papers*, 3: 199.

on the opposition of real-estate and construction interests and still more on the conflicting objectives of government policy makers and their unwillingness to allocate sufficient funds to enable much progress to be made. In 1937 Roosevelt established the Farm Security Administration to coordinate the various New Deal rural rehabilitation programs, but again relatively little was accomplished. Large sums were made available to help tenants buy their own farms, but local agents, concerned for the sake of their own records with making sure that the money was repaid, tended to make loans to tenants who were better off rather than to the most poverty stricken. While many families benefited, the overall impact upon American agriculture was negligible. The sums spent were measured in the millions, whereas, as one critic put it, only if billions had been appropriated could "the drift into tenancy and degradation be stopped and reversed." In Germany the very success of the Nazis in ending unemployment and their post-1936 drive to build their war machine created a shortage of industrial labor that made a meaningful back-to-the-soil movement impossible.<sup>29</sup>

There were significant differences between the objectives of American and National Socialist agricultural policies, the former, for example, seeking to limit output, the latter to increase it. All in all, the New Deal was the more successful in solving farm problems; far less was accomplished in Germany toward modernizing and mechanizing agriculture during the thirties. On the other hand, Nazi efforts in behalf of farm laborers were more effective than those of the New Deal; the AAA programs actually hurt many American agricultural laborers and also tenants and sharecroppers. In both nations agricultural relief brought far more benefits to large landowners than to small.<sup>30</sup>

The complications of German and American monetary and fiscal policies during the depression and of questions relating to foreign trade preclude their detailed discussion here. I shall only mention a few common themes. Both nations increased government control of the banking system but did not nationalize the banks. Both, following the precepts of economic nationalism, sought to improve the competitive position of their export industries, the Americans by devaluing the dollar, the Germans by subsidies, both by sequestering the national gold supplies and prohibiting the export of gold. Both paid most of the costs of their recovery programs by deficit

<sup>29</sup> Schweitzer, *Big Business*, 202-65; Arthur Schweitzer, "On Depression and War: Nazi Phase," *Political Science Quarterly*, 62 (1947): 332-38; Ludovici, *Das deutsche Siedlungswerk*, 1-12, 66-85; Baldwin, *Poverty and Politics*, 212, 218; Shapiro, "Decentralist Intellectuals," 950-51.

<sup>30</sup> Edwin Nourse, Joseph Davis, and John D. Black, *Three Years of the Agricultural Adjustment Act* (Washington, 1937); David E. Conrad, *The Forgotten Farmers: The Story of Sharecroppers in the New Deal* (Urbana, 1966); Schoenbaum, *Hitler's Social Revolution*, ch. 5. See also, Karl D. Bracher, Wolfgang Sauer, and Gerhard Schulz, *Die nationalsozialistische Machtergreifung* (Cologne, 1960), 570-78.

financing. However, both also ignored the newly developing Keynesian economics and remained inordinately fearful of inflation.<sup>31</sup>

Nazi and New Deal policies were not essentially different from those of other industrial nations in these respects. However, they adopted them sooner and pursued them more vigorously than, for example, the British or the French. Thus when Roosevelt decided against international stabilization of foreign exchange rates and thus “torpedoed” the London Economic Conference, the British and French were bitterly disappointed but the Germans were delighted. Roosevelt’s opinion, expressed in his “bomb-shell” message, that “the sound internal economic situation of a nation is a greater factor in its well-being than the price of its currency,” was Nazi orthodoxy. In a radio message beamed to the United States the German foreign minister, Konstantin von Neurath, praised Roosevelt’s “fearlessness” and spoke of the “heroic effort of the American people . . . to overcome the crisis and win a new prosperity.” Reichsbank president Hjalmar Schacht told a *Völkischer Beobachter* reporter that FDR had adopted the philosophy of Hitler and Mussolini: “Take your economic fate in your own hand and you will help not only yourself but the whole world.”<sup>32</sup>

In the early months of the New Deal Roosevelt toyed with the idea of stimulating exports by means of subsidized dumping and by barter agreements, trade tactics that the Nazis adopted wholeheartedly. A mighty behind-the-scenes battle was fought within the administration in 1933 and 1934 between supporters of this approach and those who believed in lowering tariff barriers by making reciprocal trade agreements based on the unconditional most-favored-nation principle. Rexford Tugwell, Raymond Moley, and George W. Peek argued the former position; the secretary of state, Cordell Hull, and the secretary of agriculture, Henry Wallace, the latter; and the president—after one of his typical attempts to get the protagonists to reconcile the irreconcilable—finally sided with Hull and Wallace. Roosevelt nevertheless continued for some time to flirt with the idea of bilateral agreements, especially one suggested by Schacht involving 800,000 bales of American cotton. The rejection of this and similar proposals resulted more from Roosevelt’s growing political and moral distaste for Hitlerism than from economic considerations. But despite Roosevelt’s

<sup>31</sup> Erbe, *Wirtschaftspolitik*, 166–72; Schweitzer, *Big Business*, 342–52; James M. Burns, *Roosevelt: The Lion and the Fox* (New York, 1956), 328–36; Robert Lekachman, *The Age of Keynes* (New York, 1968), 113–16, 138–43; Perkins, *Roosevelt I Knew*, 225–26; J. Ronnie Davis, *The New Economics and the Old Economists* (Ames, Iowa, 1971), 151–53; Rexford G. Tugwell, *The Democratic Roosevelt* (New York, 1957), 373–75; Roy F. Harrod, *The Life of John Maynard Keynes* (London, 1951), 448–49.

<sup>32</sup> Rosenman, *Public Papers*, 2: 264–65; Leuchtenburg, *Roosevelt and the New Deal*, 202–03; Maurice Vaisse, “Le mythe de l’or en France: les aspects monétaires du New Deal vus par les Français,” *Revue d’Histoire Moderne et Contemporaine*, 16 (1969): 464–65; Hans-Jürgen Schröder, *Deutschland und die Vereinigten Staaten, 1933–1939: Wirtschaft und Politik in der Entwicklung des Deutsch-Amerikanischen Gegensatzes* (Wiesbaden, 1970), 88, 91. See also, Hjalmar Schacht, *76 Jahre meines Lebens* (Bad Wörishofen, 1953), 395.



antifascism and the internationalist, free-trade rhetoric of the reciprocal trade program, it seems clear that New Deal foreign policy was as concerned with advancing national economic interests as was German policy. State Department alarm at Nazi "penetration" of Latin America, publicly expressed in strategic and moral terms, had a solid base in lost and threatened markets for American exports.<sup>33</sup>

THERE REMAINS the question of leadership, that is, of the personal roles of Roosevelt and Hitler in their nations' campaigns against the Great Depression. To overemphasize Roosevelt and Hitler as individuals would be to approach the problem simplistically, but certain parallels merit examination. It cannot be proved that neither would have achieved national leadership without the depression, but the depression surely contributed to the success of each. Yet on the surface they seem most improbable leaders of the two countries at that particular time. In an economic crisis of unprecedented severity, neither had a well-thought-out plan. Both lacked deep knowledge of or even much interest in economics.<sup>34</sup>

It is no less than paradoxical that the American electorate, provincial in outlook, admiring of self-made men and physical prowess, and scornful of "aristocrats," should, at a time when millions were existing on the edge of starvation, choose for president a man who lived on inherited wealth, who came from the top of the upper crust, who had been educated in the swankest private schools, who had a broad cosmopolitan outlook, and (to descend to a lesser but not politically unimportant level) who was a cripple. But no more a paradox than that a country whose citizens

<sup>33</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 155-59, 253; Hans-Jürgen Schröder, "Die Vereinigten Staaten und die nationalsozialistische Handelspolitik gegenüber Lateinamerika 1937/38," *Jahrbuch für Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas*, 7 (1970): 309-23, 355-57; Arnold A. Offner, *American Appeasement: United States Foreign Policy and Germany, 1933-1938* (Cambridge, Mass., 1969), 99-102; Lloyd C. Gardner, *Economic Aspects of New Deal Diplomacy* (Boston, 1971), 42-43, 100; Cordell Hull, *The Memoirs of Cordell Hull* (New York, 1948), 1: 353-77; Schlesinger, *Coming of the New Deal*, 187-88, 191-93, 255-58. The reciprocal trade program, despite the high hopes of its supporters, had little effect upon American trade or the world economy. Furthermore the argument of Secretary Hull and others that the "non-discriminatory" reciprocal trade policy was particularly high-minded and that bilateralism and barter agreements were per se destructive of the interests of underdeveloped nations makes little sense. See Schlesinger, *ibid.*, 259-60; and Mandelbaum, "Experiment in Full Employment," 186-87.

<sup>34</sup> Mason, "Primacy of Politics," 173-74; Perkins, *Roosevelt I Knew*, 34; Burns, *Roosevelt*, 329, 334-36. Even Fustfeld, who argues that Roosevelt "had a well-developed economic philosophy," admits that it was not derived from the writings of economists or other thinkers. Roosevelt's own opinion is perhaps revealed in a remark he made to Marriner Eccles after listening to a debate between Eccles and a conservative senator: "You made the problem so simple that even I was able to understand it." Fustfeld, *Economic Thought of FDR*, 5; Davis, *New Economics*, 152. John D. Heyl argues that "Hitler's economic ideas are worth considering," but in essence claims little more than that "Hitler acquired a certain familiarity with economic issues." And he quotes Hitler as saying in 1934: "Don't allow yourself to be deceived by cut-and-dried [economic] theories. Certainly I know less today about these matters than I thought I knew a few years ago." John D. Heyl, "Hitler's Economic Thought: A Reappraisal," *Central European History*, 6 (1973): 83, 92.

were supposed to have an exaggerated respect for hard work, for education and high culture and family lineage, and who had a reputation for orderliness and social discipline should follow the lead of a high-school dropout, a lazy ne'er-do-well, a low-born Austrian who could not even speak good German, the head of a rowdy movement openly committed to disorder and violence. Equally strange, Roosevelt and Hitler appealed most strongly to their social and economic opposites: Roosevelt to industrial workers, to farmers, to the unemployed and the rejected; Hitler to hard-working shopkeepers and peasants, and, eventually, to industrialists, great landowners, and the military.

It may of course be true that these seeming contradictions are of no significance. Probably any Democrat would have defeated Hoover in 1932, and although Hitler became chancellor in a technically legal way, his subsequent seizure of total power was accomplished without the consent, if not necessarily without the approval, of a majority of the German people. Yet the personal impact of Roosevelt and Hitler on the two societies in the depths of the Great Depression was very large. Their policies aside, both exerted enormous psychological influence upon the citizenry. Roosevelt's patrician concern for mass suffering, his charm, his calm confidence, his gaiety, even his cavalier approach to the grave problems of the day had, according to countless witnesses, an immediate and lasting effect upon the American people. Hitler's resentment of the rich and well born, however psychotic in origin, appealed powerfully to millions of Germans. His ruthless, terrifying determination, always teetering on the edge of hysteria, combined with the aura of encapsulated remoteness that he projected to paralyze those who opposed him, to reduce most of his close associates to sycophancy, and to inspire awe among masses of ordinary Germans. Both the euphoria of the Hundred Days and the nationalistic fervor that swept Germany in the early months of 1933 made millions almost incapable of thought, let alone of judgment. Bills swept through Congress ill drafted and scarcely debated, basic rights were abolished in Germany without even an attempt at resistance, and both were possible largely because of the personalities of the two leaders.<sup>35</sup>

Much of this was probably spontaneous, but not all. Roosevelt and Hitler employed the latest technologies to dramatize themselves and to influence public opinion. Roosevelt's flight to Chicago to accept the Democratic nomination and Hitler's whirlwind tours testify to their swift grasp of the psychological as well as the practical value of air travel to politicians. And no greater masters of the radio ever lived—Roosevelt with his low-keyed, fatherly, intimate fireside chats, Hitler with his shrill harangues beneath the massed swastikas at Nuremberg. Both were terrible

<sup>35</sup> See, among many examples, Leuchtenburg, *Roosevelt and the New Deal*, 44-45, and Helmut Krausnick, "Stages of Co-ordination," in Lawrence Wilson, tr., *The Road to Dictatorship* (London, 1964), 132-33.

administrators in the formal sense but virtuosos at handling subordinates. Their governments were marked by confusion, overlapping jurisdictions, and factional conflicts, yet somehow they transformed these inadequacies into political assets—symbols not of weakness or inefficiency but of energy and zeal in a time of grave emergency.<sup>36</sup>

Both also made brilliant use of the crisis psychology of 1933, emphasizing the suffering of the times rather than attempting to disguise or minimize it. "The misery of our people is horrible," Hitler said in his first radio address after becoming chancellor. "To the hungry unemployed millions of industrial workers is added the impoverishment of the whole middle class and the artisans. If this decay also finally finishes off the German farmers we will face a catastrophe of incalculable size."<sup>37</sup> Roosevelt's personal style was more reassuring than alarmist, but he also stressed the seriousness of the situation and the urgent need for decisive action: "Action, and action now," as he put it in his inaugural.

Both the Roosevelt and Hitler governments tried to influence public opinion in new and forceful ways. Roosevelt did not create a propaganda machine even remotely comparable to Goebbels's, but under the New Deal the government undertook efforts unprecedented in peacetime to sell its policies to the public. The NRA slogan "We Do Our Part" served the same function as the Nazis' incessantly repeated *Gemeinnutz geht vor Eigennutz*. With Roosevelt's approval, General Hugh Johnson, head of the NRA and designer of its Blue Eagle symbol, organized a massive campaign to rally support for the NRA. "Those who are not with us are against us," Johnson orated, "and the way to show that you are a part of this great army of the New Deal is to insist on this symbol of solidarity." Johnson denounced "chiselers" and "slackers"; his office plastered the land with billboard displays; distributed posters, lapel buttons, and stickers; dispatched volunteer speakers across the country; and published *Helpful Hints* and *Pointed Paragraphs* to provide them with The Word. Roosevelt

<sup>36</sup> Judgments of Roosevelt's and Hitler's abilities as administrators are of course highly subjective. Furthermore the internal workings of any government seem confused when examined in detail. It is clear nevertheless that both Roosevelt and Hitler were exceptionally prone to set up confused lines of responsibility among their subordinates and to tolerate and at times encourage interdepartmental and intradepartmental rivalries. Waste and ineffectiveness frequently resulted. Thus, Burns writes: "Again and again Roosevelt flouted the central rule of administration that the boss must co-ordinate the men and agencies under him. . . . [He] put into the same office or job men who differed with each other in temperament and viewpoint." Bracher concludes that "friction, waste, duplication" were deliberate Hitlerian techniques, whereas Fischer argues that when Hitler "made at least two and most often many more boards, agencies, and men responsible for each assignment" he was merely revealing the senselessness and lack of guiding principles of his system, but they (and other scholars) agree as to the facts. Burns considers Roosevelt "an artist in government," master of the technique of divide and rule, whose "first concern was power," the subjection of the bureaucracy to executive control. Bracher writes that Hitler displayed "matchless virtuosity" in making others dependent upon him. "The Leader was the sole figure standing above the confusion of jurisdictions and command chains." Burns, *Roosevelt*, 371–75; Bracher, *German Dictatorship*, 212; Fischer, *Deutsche Wirtschaftspolitik*, 77.

<sup>37</sup> Domarus, *Reden und Proklamationen*, 1: 192.



*Fig. 1.* Photograph by Dorothea Lange. Evicted sharecropper from Arkansas, 1936.  
Library of Congress.





*Fig. 2. Painted in Germany, 1930s. Artist unidentified. Photograph courtesy  
Suddeutscher Verlag, Munich.*

# IS HOUSING A PUBLIC RESPONSIBILITY?

## Government Money & Regulation Guarantee...

### FREE SCHOOLS

State and federal governments spend two and a half billion dollars annually for public education.



### CHEAP POSTAL SERVICE

The postal system has cost the American taxpayer an average of more than 125 million dollars a year for the last five years.



### BETTER TRAINS

State and federal governments have given more than a billion dollars to the railroads, in the form of land grants and other aids. Gifts of land totalled more than 175 million acres, an area larger than the state of Texas.

## Is the American Home LESS IMPORTANT?



### BETTER SHIPS

The government today is paying about \$10,000 per ton annually for the carrying of war materials which would cost, under normal ocean rates, only \$3,000,000. The difference, \$7,000,000 is a subsidy, and nothing but a subsidy.

Wholesale Price of War Materials, 1935

### BETTER HIGHWAYS

The federal government has spent an average of 750 million dollars a year on highways for the last five years.



### BETTER AIR LINES

To aid American airlines, the federal government has paid an average of nearly 17 million dollars annually for the last five years.



Fig. 3. Poster from a traveling series of information panels prepared by the U.S. Resettlement Administration, 1935. Library of Congress.





Fig. 4. "Our last hope: Hitler." German election poster, 1932. Library of Congress.

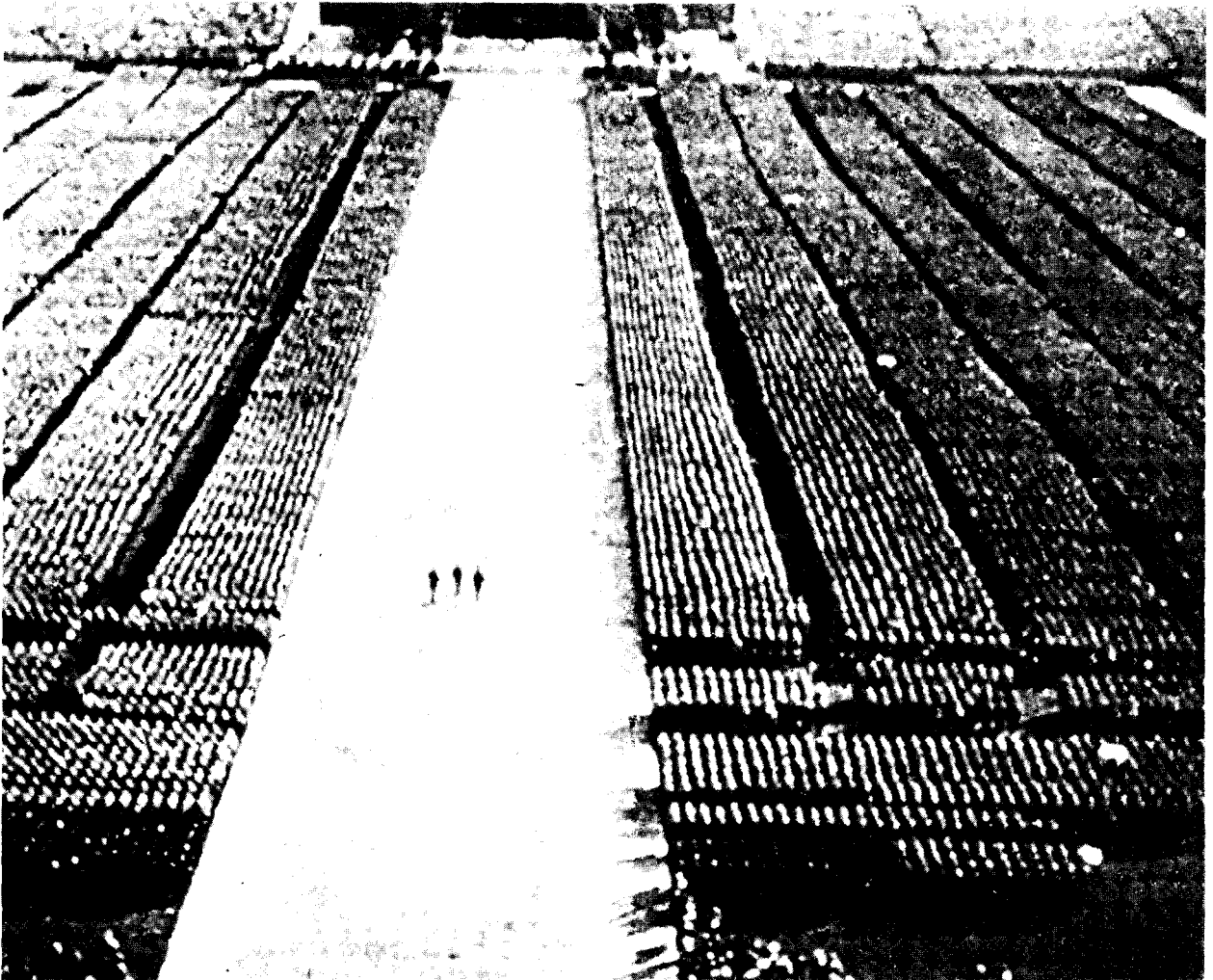


Fig. 5. Hitler reviewing troops during Nuremberg rally, 1936. Still from *Triumph of the Will*. Photograph courtesy Museum of Modern Art, Film Stills Archive.

himself, in a fireside chat, compared the Blue Eagle to a “bright badge” worn by soldiers in night attacks to help separate friend from foe. Placed beside the awesome Nazi displays at Nuremberg, even the ten-hour, 250,000-person NRA parade up Fifth Avenue in September 1933 may seem insignificant, but it and other NRA parades and hoopla were designed to serve the same functions: rousing patriotic feelings and creating in the public mind the impression of so extensive a support for government policies as to make disagreement appear close to treason. As Johnson himself explained, the purpose was to “put the enforcement of this law into the hands of the *whole* people.”<sup>38</sup>

<sup>38</sup> Hawley, *New Deal and Monopoly*, 53–55; Schlesinger, *Coming of the New Deal*, 112–16; Hugh S. Johnson, *The Blue Eagle from Egg to Earth* (New York, 1935), 255–58, 261, 264; Rosenman, *Public Papers*, 2: 301; Leuchtenburg, “Analogue of War,” 121, 133–34. A 1935



Fig. 6. Part of a battalion of tractors. Still from *The Plow That Broke the Plains*, 1936. Photograph courtesy Museum of Modern Art, Film Stills Archive.

Another example of New Deal propaganda is provided by the efforts of the Resettlement Administration and the Farm Security Administration under Rexford Tugwell. Because Pare Lorenz's government-sponsored films, *The Plow That Broke the Plains* (1936) and *The River* (1938), and the still photographs of Dorothea Lange, Walker Evans, Margaret Bourke-White, Gordon Parks, and others were esthetic achievements of the highest order, we tend to forget that they were a form of official advertising designed to explain and defend the New Deal approach to rural social and economic problems. They differed from Leni Riefenstahl's *Triumph of the Will* (also a cinematic masterpiece) and the annual volumes of photographs celebrating National Socialism chiefly in style—"soft" rather than "hard" sell—and point of view.<sup>39</sup>

Brookings Institution study of the NRA put it this way: "The work of 'selling' it [the NRA] to the country brought into play demonstrations of emotionalism, pageantry, and oratorical appeals usually associated with war-time propaganda rather than with the ordinary functionings of peace-time government." Leverett S. Lyon, et al., *The National Recovery Administration: An Analysis and Appraisal* (Washington, 1935), 52.

<sup>39</sup> Robert L. Snyder, *Pare Lorentz and the Documentary Film* (Norman, Okla., 1968), 21-95; Baldwin, *Poverty and Politics*, 118-19; Siegfried Kracauer, *From Caligari to Hitler: A Psychological History of the German Film* (Princeton, 1970), 297-303; Paul Rotha, *Documentary*

The New Deal efforts at mass persuasion were unparalleled among democracies in peacetime—nothing comparable was attempted in France or Great Britain before the outbreak of war in 1939.<sup>40</sup> They reflect the attitude of the Roosevelt government, shared by Hitler's, that the economic emergency demanded a common effort above and beyond politics. The crisis justified the casting aside of precedent, the nationalistic mobilization of society, and the removal of traditional restraints on the power of the state, as in war, and it required personal leadership more forceful than that necessary in normal times. That all these attitudes were typical of Hitler goes without saying, but Roosevelt held them too. Consider this passage in his first inaugural:

I assume unhesitatingly the leadership of this great army of our people. . . . Our true destiny is not to be administered unto but to minister to ourselves. . . . In the event that Congress shall fail . . . I shall ask the Congress for the one remaining instrument to meet the crisis—broad executive power to wage a war against the emergency, as great as the power that would be given to me if we were in fact invaded by a foreign foe.<sup>41</sup>

Roosevelt was neither a totalitarian nor a dictator, real or potential, but his tactics and his rhetoric made it possible for anti-New Dealers and outright fascists to argue that he was both. Many of the accusations of conservatives and Communists in the United States were politically motivated, as were, of course, Nazi comments on the president. But during the first years of the New Deal the German press praised him and the New Deal to the skies. Before Hitler came to power he was, although impressed by Henry Ford's automobiles and the racially oriented American immigration laws, basically contemptuous of the United States, which he considered an overly materialistic nation dominated by Jews, "millionaires, beauty queens, stupid [phonograph] records, and Hollywood."<sup>42</sup>

*Film* (New York, 1963), 199–200, 308, 317–19. See also Erich Koch, ed., *Nürnberg 1934: Ein Bildbericht vom Parteitag 1934* (Berlin [n.d.]). In 1934 Harry Hopkins negotiated a contract with Pathé News for a series of short commercial films for the WPA. This led *Newsweek* to comment: "New Deal Goes Hollywood." Snyder, *Pare Lorentz*, 12–13.

<sup>40</sup> As early as 1908 the Department of Agriculture was producing short educational and instructional movies, and the Department of the Interior and other government agencies also made such films in considerable numbers beginning in 1911. These were not, however, designed for general distribution, as both *The Plow* and *The River* clearly were. During the 1920s and early 1930s the British Empire Marketing Board and later the British Post Office produced films advertising their activities, but not for commercial distribution. Snyder, *Pare Lorentz*, 8–11; Rotha, *Documentary Film*, 96–99; Dartington Hall Trustees, *The Factual Film* (London, 1947), 43–45, 51–52.

<sup>41</sup> Rosenman, *Public Papers*, 2: 14–15. This last sentence evoked the loudest cheering Roosevelt's speech produced. Eleanor Roosevelt found the response "a little terrifying." Commenting on it later, she said: "You felt they would do *anything*—if only someone would tell them *what* to do." Lash, *Eleanor and Franklin*, 360. Leuchtenburg writes: "Roosevelt personified the state as protector." *Roosevelt and the New Deal*, 331. For a full discussion of the "war psychology" of New Deal policies, see Leuchtenburg, "Analogue of War," 81–143.

<sup>42</sup> Gerhard L. Weinberg, "Hitler's Image of the United States," *AHR*, 69 (1963–64): 1008–09; Joachim Remak, "Hitlers Amerikapolitik," *Aussenpolitik*, 6 (1955): 706–08; Offner, *American Appeasement*, 13–14, 32–33.



Nevertheless, he and his party were impressed by New Deal depression policies. "Mr. Roosevelt . . . marches straight to his objectives over Congress, lobbies, and the bureaucracy," Hitler told Anne O'Hare McCormick of the *New York Times* in July 1933. In July 1934 the *Völkischer Beobachter* described Roosevelt as "absolute lord and master" of the nation, his position "not entirely dissimilar" to a dictator's. Roosevelt's books, *Looking Forward* (1933) and *On Our Way* (1934) were translated into German and enthusiastically reviewed, the critics being quick to draw attention to parallels in New Deal and National Socialist experiences.<sup>43</sup>

A friendly German biography, *Roosevelt: A Revolutionary with Common Sense*, by Helmut Magers, appeared in 1934. Magers described the New Deal as "an authoritarian revolution," a revolution "from above," and pointed up what he called the "surprising similarities" it bore to the Nazi revolution. That there appeared to be some basis for this view at the time is suggested by the fact that Ambassador William E. Dodd wrote a foreword to Magers's book in which he praised the author's "outstanding success" in describing both conditions in the United States and the nation's "unique [*einzigartig*] leader" and spoke of the "heroic efforts being made in Germany and the United States to solve the basic problem of social balance."<sup>44</sup>

Dodd was vehemently anti-Nazi, but he hoped that German moderates like Schacht and Neurath would be able to overthrow Hitler or at least restrain him. He considered the Magers volume an "excellent, friendly, unpartisan book . . . without a sentence that could have been quoted to our disadvantage" and allowed his foreword to be published despite State Department objections. The Germans, for their part, went out of their way to welcome Dodd. A throng of reporters and Foreign Office officials greeted him when he arrived in Berlin. He was put up in the six-room royal suite of the Hotel Esplanade and charged only ten dollars a day. He was invited to lecture at the University of Munich. Hitler assured him that Germany had no warlike intentions. When he criticized authoritarian rule and economic nationalism in a speech, the German press reported his remarks fairly and accurately.<sup>45</sup>

At the end of Roosevelt's first year in office Hitler sent him a message through diplomatic channels offering sincere congratulations for "his

<sup>43</sup> *New York Times*, July 10, 1933, quoted in James V. Compton, *The Swastika and the Eagle: Hitler, the United States, and the Origins of World War II* (Boston, 1967), 20; Schröder, *Deutschland und die Vereinigten Staaten*, 99–114. Mussolini wrote a widely publicized review of *Looking Forward* in which he noted a number of similarities between Roosevelt's thinking on economic policy and his own. He concluded, however, that while Roosevelt's ideas were superficially related to "fascist Corporatism . . . it would be an exaggeration to say anything more." John P. Diggins, *Mussolini and Fascism: The View from America* (Princeton, 1972), 281.

<sup>44</sup> Helmut Magers, *Roosevelt: Ein Revolutionär aus Common Sense* (Leipzig, 1934), 5, 10, *passim*.

<sup>45</sup> Robert Dallek, *Democrat and Diplomat: The Life of William E. Dodd* (New York, 1968), 196–211, 225, 227. Dallek incorrectly implies that Dodd yielded to State Department pressure with regard to his foreword to Magers's biography.

heroic efforts in the interests of the American people. The President's successful battle against economic distress is being followed by the entire German people with interest and admiration," Hitler announced. In November 1934 the *Völkischer Beobachter* characterized Democratic gains in the Congressional elections as an "exceptionally personal success" for Roosevelt. The tone of this article was almost worshipful, the rhetoric hyperbolic. The president (a man of "irreproachable, extremely responsible character and immovable will" [*tadelnsfreie verantwortungsvolle Gesinnung und . . . unverrückbarer Wille*]) had shown himself to be a "warmhearted leader of the people with a profound understanding of social needs" as well as an energetic politician.<sup>46</sup> This attitude ended in 1936, although even after Roosevelt's "quarantine" speech the Nazi propaganda machine refrained for tactical reasons from attacking him personally. It is clear, however, that early New Deal depression policies seemed to the Nazis essentially like their own and the role of Roosevelt not very different from the Führer's.<sup>47</sup>

The importance of these leaders in the fight against the depression lies less in what they did to revive the economy than in the shift in public mood they triggered. In early 1933 that mood was profoundly pessimistic. For four years business conditions had been growing almost steadily worse. The promises and optimistic predictions of innumerable political and business leaders that the tide would turn had all proved illusory. Millions had lost not merely their jobs and savings, but hope itself. It was the duration more than the depth of the decline that was truly depressing.

The Great Depression was totally unlike any earlier economic slump. Men had noted as early as the eighteenth century that economic activity tended to rise and fall in recurrent patterns, and during the nineteenth century the concept of the business cycle was firmly established. Cycles were variously explained, and the terminology was not precise, but it was accepted that the world economy moved in an irregular but unending path through periods of expansion, crisis or panic, recession or depression, and then returned to expansion. Before the collapse of the 1930s a cycle was usually identified by its most dramatic phase, the crisis or panic: witness the American "panics" of 1819, 1837, 1857, 1873, 1893, and 1907. The business slump that followed panics was characteristically

<sup>46</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 112; *Foreign Relations of the United States: Diplomatic Papers, 1934* (Washington, 1951), 2: 419; *Völkischer Beobachter*, Nov. 9, 1934. I am indebted to Professor Hans-Jürgen Schröder of the University of Mainz for copies of this and other articles about the New Deal that appeared in the German press.

<sup>47</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 118-19. In May 1940 *Das Reich* published an article comparing Nazi and New Deal policies to combat the depression: "Hitler and Roosevelt: A German Success—An American Attempt." The anonymous author blamed what he called the weaknesses of the New Deal not on Roosevelt but on the "sacrosanct Constitution" of the United States and on the "parliamentary-democratic system" that forced Roosevelt to cater to conflicting interests. "We began with an idea and carried out the practical measures without regard for consequences. America began with many practical measures that without inner coherence covered over each wound with a special bandage." *Das Reich*, May 26, 1940.



precipitous but mercifully brief. In his classic study of *Business Cycles* (1913), Wesley Clair Mitchell wrote: "The lowest ebb of the physical volume of industrial production usually comes in either the first or the second year after a severe crisis."<sup>48</sup> The German and French words *die Krise* and *la crise* reflect this same focus on the panic aspect of the "normal" business cycle. However, the recession that came after the "panic of 1929" did not follow the expected pattern. Interminably, or so it seemed, it continued. By the end of 1932 industrial production in both the United States and Germany was scarcely more than half of what it had been in 1929.

By the early 1930s professional economists were beginning to realize that the character of business cycles was changing. The first edition of the *Encyclopedia of the Social Sciences*, published between 1930 and 1935, contains an article on *crises* by Jean Lescure, a French expert on business cycles. "A crisis," Lescure wrote, "may be defined as a grave and sudden disturbance of economic equilibrium." Lescure went on to discuss the nature and history of crises, paying special attention to the impact of industrialization and of the growth of cartels and trusts, which he believed had reduced the acuteness of crises but also delayed the process of recovery. "It would seem," he concluded, "that for the term crisis one may henceforth substitute that of depression; it is reasonable to speak today of a world depression rather than of a world crisis."<sup>49</sup>

Lescure's emphasis on the word "depression" highlights the psychological impact of the long economic decline, the pessimism, the sense of hopelessness that had little to do with the size of an individual's pocketbook. A constricting pall appeared to have descended upon the world. Among economists, stagnation theorists flourished and learned authorities spoke of a "mature" economy and the end of the era of economic growth spawned by the Industrial Revolution. Many recommended what the French called a "Malthusian" approach, the reduction of output to the current level of consumption rather than the attempt to increase consumption.<sup>50</sup> Governments, faced with the most extended fall in prices since the 1890s, responded not with inflationary measures but by adopting deflationary monetary policies and by slashing already shrunk budgets.<sup>51</sup>

<sup>48</sup> Wesley C. Mitchell, *Business Cycles and their Causes* (Berkeley, 1963), 133. The general acceptance of this theory helps to explain President Hoover's often-derided optimistic prognostications in 1930 and 1931. Businessmen, according to Joseph Schumpeter, were "by no means over-pessimistic" even in late 1930. The collapse of the American banking system in early 1933, Schumpeter claimed, finally destroyed hope. "The psychic framework of society, which till then had borne up well, was at last giving way." Schumpeter, *Business Cycles*, 2: 906, 944.

<sup>49</sup> Jean Lescure, "Crises," in E. R. A. Seligman and Alvin Johnson, eds., *Encyclopedia of the Social Sciences* (New York, 1930-35), 4: 595-99.

<sup>50</sup> Dorfman, *Economic Mind in American Civilization*, 5: 723-25; Alfred Sauvy, *Histoire économique de la France entre les deux guerres* (Paris, 1967), 2: 359-67. For a brief summary of the mood of early New Deal economists, see Schlesinger, *Coming of the New Deal*, 180-81.

<sup>51</sup> "The deflationary spiral was, in most countries, accentuated by orthodox Government financial policy as then conceived. The automatic effect of a depression was to reduce tax

Businessmen feared to make new investments. Trade languished. Unions dealt with mounting unemployment by urging that youths be kept longer in school, that working women return to the home, that older men retire early. The mood of unemployed workers, some twenty million in the United States and Germany alone by early 1933, was more apathetic than rebellious.<sup>52</sup> It was this general pall of despair and listlessness that the New Deal and the Nazi revolution, personified by Roosevelt and Hitler, dispelled. Long before their economic policies had much effect on the stalled business cycle, they had revitalized the two societies.

COMPARISON WITH Great Britain and France is suggestive. Economic historians disagree about the character of Britain's economic recovery in the 1930s, but the argument concerns the growth *rate* and its causes, not the fact of expansion.<sup>53</sup> By 1937 industrial output was over thirty per cent larger than in 1933, and unemployment had been almost halved. Even allowing for the facts that the British economy had been sluggish in the 1920s, unemployment extremely high, and, therefore, that the world depression seemed a less dramatic collapse in Britain than it did in the United States or Germany, the improvement between 1933 and 1937 marked a very substantial change.<sup>54</sup>

Furthermore, during these years the British government made many efforts to improve conditions. To aid industry it adopted tactics strikingly similar to those of the NRA. It allowed coal operators to limit and allocate output, fix prices, and amalgamate companies; it encouraged cotton textile manufacturers to scrap inefficient machinery, and the steel industry to cartelize its operations. Both the ailing shipbuilding industry and the healthy automobile industry received government subsidies. Electric utility companies were assisted in consolidating their activities by the Central Electricity Board. The remarkable British housing boom, it is true, was largely the work of private enterprise, but government construction was significant—and focused where it was most needed, on slum clearance and homes for the poor. Agriculture was also assisted through a complex mixture of import quotas, tariffs, subsidies, and marketing schemes.

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revenue and increase expenditure for the relief of the unemployed, and hence to produce a budgetary deficit. Orthodox finance demanded that the budget should be balanced annually, by rigid economies in expenditure and the imposition of additional taxes." H. W. Arndt, *The Economic Lessons of the Nineteen-Thirties* (Oxford, 1944), 254.

<sup>52</sup> For convenient discussions of union attitudes in various countries, see the essays in Denise Fauvel-Rouif, ed., *Mouvements ouvriers et dépression économique de 1929 à 1939* (Assen, 1966). On the attitudes of unemployed workers, see Philip Eisenberg and Paul F. Lazarsfeld, "The Psychological Effects of Unemployment," *Psychological Bulletin*, 35 (1938): 358–90.

<sup>53</sup> The most recent summary of the controversy is B. W. E. Alford, *Depression and Recovery? British Economic Growth, 1918–1939* (London, 1972).

<sup>54</sup> G. D. A. MacDougall, "General Survey, 1929–1937," in British Association, *Britain in Recovery* (London, 1938), 1–84; Charles L. Mowat, *Britain Between the Wars: 1918–1940* (Boston, 1971), 432–35.

The government also acted to help British labor. Unemployment insurance and relief services were well established long before 1933, but beginning with the Unemployment Act of 1934 the system was considerably improved. Insurance was put on a sounder financial basis, and an Unemployment Assistance Board was set up to administer the relief program. In 1936 half a million agricultural wage earners were brought into the insurance system. Other laws sought to encourage the movement of labor from the economically stagnant north and west to the more prosperous southeast, and manufacturers willing to build factories in the depressed areas received subsidies.<sup>55</sup> Economic recovery was thus accompanied by considerable social reform; by 1937 the combination of a progressive tax structure and extended social services was transferring five or six per cent of the national income from rich to poor, raising the real income of the working classes by some eight to fourteen per cent. In the United States, by way of contrast, New Deal legislation had almost no measurable effect on income distribution.<sup>56</sup>

Yet the people of Great Britain had no sense of experiencing a new era. It seems clear (although such things are difficult to measure) that the national mood remained depressed, despite economic progress. No political leader was able to generate a sense of common commitment to the battle against the depression. When David Lloyd George announced a plan for a "New Deal for Britain" in 1935—it was little more than a rehash of proposals he had made repeatedly in the 1920s—Ramsay MacDonald's cabinet was thrown almost into a panic and gave serious consideration to inviting both Lloyd George and Winston Churchill to join the government, but it did not do so. Chancellor of the Exchequer Neville Chamberlain wrote in his diary at this time: "The P.M. [MacDonald] is ill and tired, S[tanley] B[aldwin] is tired and won't apply his mind to problems." (Contrast this state of mind with the mood in government circles in Washington and Berlin.) "It is certainly time there was a change," Chamberlain also wrote, having himself in mind as the person to institute it. But after Chamberlain became prime minister in 1937, the national mood was no different. Chamberlain was a conservative of the finest type, hard working and public spirited; no one in Great Britain contributed more to social reform in the interwar years. But he was by this time also aging and ailing—unable to inspire public enthusiasm. As he said of himself, he could not "unbutton."<sup>57</sup>

<sup>55</sup> A. J. Youngson, *Britain's Economic Growth: 1920-1966* (2d ed.; London, 1968), 96-120, 127-31; Bentley B. Gilbert, *British Social Policy: 1914-1939* (Ithaca, 1970), 178-88, 191, 252-54. Although critical of the limitations of British social policy and of most of the politicians of the period, Gilbert concludes that by 1939 "the British State had committed itself to the maintenance of all its citizens according to need as a matter of right" (p. 308). See also, H. W. Richardson, *Economic Recovery in Britain, 1932-9* (London, 1967).

<sup>56</sup> Mowat, *Britain between the Wars*, 492; Youngson, *Britain's Economic Growth*, 136-37; Simon Kuznets, *National Income: A Survey of Findings* (New York, 1946), 99.

<sup>57</sup> Gilbert, *British Social Policy*, 185; Keith Feiling, *The Life of Neville Chamberlain* (London, 1947), 242-43.

Thus the 1930s passed into memory in Great Britain as a time of inactivity and decline—the Great Slump. “The man in the street’s view of Britain’s experience . . . is that activity was stagnant and very depressed,” the economic historian Harry W. Richardson writes—this even though “any glance at the evidence shows it to be a misconception.”<sup>58</sup> Surely this helps to explain why fascism became a more formidable force in Britain than in the United States, despite economic recovery and the far more serious and immediate threat that the Nazis posed to the British.

The French experience provides another opportunity to study this aspect of the depression. Its full force struck France late, but by 1935 conditions were very bad. Farm prices had collapsed. Industrial output was down sharply. French workers were probably worse off than those of any other industrial nation: wage rates as low as eighty centimes an hour (the franc was worth about five cents) were not unknown; employers were autocratic, superficially well organized, and adamantly opposed to collective bargaining; unemployment was increasing rapidly and was far greater than French statistics indicated. Over 503,000 persons were receiving relief payments in February 1935, an increase of more than 150,000 in one year, and many of the unemployed were unable to meet all the eight “general conditions” required to qualify for aid. Furthermore, in counting the unemployed the French government made no allowance for those who had given up looking for work, for individuals who had lost their jobs and returned to family farms, or for unemployed foreigners who had no work permits. The Ministry of Labor, which compiled the unemployment statistics, itself confessed that the number of *employed* Frenchmen had declined by 1,880,000 since 1930, and this estimate was probably too low.<sup>59</sup>

Moreover, French governments, aside from the fact that no party or coalition was capable of staying in power for more than a few months, were not merely ineffective but complacent in dealing with the problems of the depression. In 1935, when organized business sought a law (*le projet Marchandeau*) much like the NRA codes enforcing restrictive trade

<sup>58</sup> Richardson, *Economic Recovery*, 21. Richardson argues that government policies had little to do with recovery and were not even primarily designed to fight the depression. “In Britain new policies were implemented either on grounds of expediency or because short-run considerations forced them on a reluctant government.” H. W. Richardson, “The Economic Significance of the Depression in Britain,” *Journal of Contemporary History*, 9 (1969): no. 4, p. 18.

<sup>59</sup> Sauvy, *Histoire économique*, 2: 111–37, 165–80; Alfred Sauvy, “The Economic Crisis of the 1930s in France,” *Journal of Contemporary History*, 9 (1969): no. 4, pp. 21–26; Simone Weil, *La condition ouvrière* (Paris, 1951), 35–108; Jean Touchard and Louis Bodin, “L’État de l’opinion au début de l’année 1936,” in *Cahiers de la Fondation Nationale des Sciences Politiques, Léon Blum: chef de gouvernement, 1936–1937* (Paris, 1967), 53–54; Georges Lefranc, ed., *Juin 36* (Paris, 1966), 209–10; Henry W. Ehrmann, *Organized Business in France* (Princeton, 1957), 15–32; Ministère du Travail, *Bulletin*, 42 (1935): 1, 7–8. See also, M. Daclin, *La crise des années 30 à Besançon* (Paris, 1968), 80–83; Walter Galenson and Arnold Zellner, “International Comparison of Unemployment Rates,” in National Bureau of Economic Research, *The Measurement and Behavior of Unemployment* (Princeton, 1957), 453, 508–26; Gabrielle Letellier *et al.*, *Le chômage en France de 1930 à 1936* (Paris, 1938), 35–42, 246–62.

agreements, the bill was defeated. The official position of the Ministry of Labor on unemployment, repeatedly enunciated in its annual reports, ran as follows: "The state cannot pretend to be able to eliminate or diminish unemployment since its activities do not get at the causes of the evil." The minister's report of June 22, 1936, praised local relief officers for their "very important" services in "not allowing unemployed workers to get aid unless they met all the requirements." Thus "public funds have been safeguarded."<sup>60</sup>

Then quite suddenly in the spring of 1936 the electoral victory of the Popular Front unleashed a spontaneous, grass-roots outburst of protest, signalized by a wave of sit-down strikes that brought the economy to a standstill. Thoroughly alarmed, the leaders of industry swiftly capitulated, throwing themselves on the mercy of the new Socialist premier, Léon Blum. Within a matter of days a new system of labor-management relations, buttressed by a host of laws similar to those of the New Deal, was hammered out by representatives of big business and the unions and pushed through parliament by the Blum government: state-supervised collective bargaining, large wage increases, a forty-hour week, and paid vacations, along with banking reform and a program to support agricultural prices were all instituted in one hectic burst of activity.<sup>61</sup>

This transformation was, of course, shortlived; by 1939 France was more divided and lacking in any sense of commitment to common national purposes than in 1935. To what extent a lack of leadership and particularly Léon Blum's personal inadequacies caused this reversal is a very difficult question. Blum's performance can be criticized from two perspectives. First of all, should he, as a lifelong socialist, have attempted to use the crisis to change France more radically? In a brilliant article, "*Tout est possible*," the left-wing socialist Marceau Pivert urged him to try. "The masses are much more advanced than we imagine," Pivert insisted. They are ready not merely for "an insipid cup of medicinal tea," but for "drastic surgery," including the nationalization of banks, utility companies, and "trusts," and the confiscation of the wealth of "deserters of

<sup>60</sup> Audré Piettre, *L'Evolution des Ententes industrielles en France depuis la Crise* (Paris, 1936), 85-93, 166-83; Ehrmann, *Organized Business in France*, 370-71; Ministère du Travail, *Bulletin*, 43 (1936): 173, 171. In February 1935 Premier Flandin explained his unemployment "policy" to the National Assembly: Large-scale direct relief on the model of the British dole would be impossible in France because of the rapid increase in French unemployment, because France "lacked the resources" of Britain, and because it would unbalance the budget. Extensive public works on the American model would not work because "the future earnings" of such projects would not equal their cost, because France lacked the necessary capital and was too deeply in debt to borrow it, and because France did not need more public works. Yet Flandin admitted that he was receiving "hundreds of heartrending letters each day" from unemployed workers. He professed to be feeling "*une angoisse quotidienne . . . de jour et de nuit*." Quoted in *ibid.*, 42 (1935): 114-19.

<sup>61</sup> Joel Colton, *Léon Blum: Humanist in Politics* (New York, 1966), 129-59; Georges Lefranc, *Histoire du Front Populaire* (Paris, 1965), 139-86. See also two excellent collections of documents and eyewitness reports, Louis Bodin and Jean Touchard, eds., *Front Populaire: 1936* (2d ed.; Paris, 1965), and Lefranc, *Jun 36*.

the franc."<sup>62</sup> Blum rejected these proposals, being backed by most of the Socialists and also by the Communists, the union leaders, and the Radicals who made up his coalition. He did so on the reasonable, indeed honorable ground that the Popular Front parties, having campaigned on a platform of moderate reform within the capitalist system, had no mandate for revolutionary change. "Our duty," he later said, "was . . . to show ourselves scrupulously faithful to the program." He felt that he must "keep loyally, publicly, the promise that I had made." But it can be argued that the upheavals of 1936 *were* revolutionary, even that (as Pierre Mendès France said many years later) the election of 1936 was not a plebiscite for any particular reform but "an affirmation of a popular desire to see the country break out of its deflationary rut and conservative structures." Socialist critics, re-reading some of Blum's modest and diffident comments about the responsibilities of his high office, have been impressed by a "crushing masochism" in his character, a defeatist attitude, an exaggerated concern for punctilio. Blum was too much the "grand bourgeois," Pivert recollected, "too subtle, too refined to be a revolutionary leader."<sup>63</sup> Georges Lefranc, both a participant in and one of the leading historians of the events of 1936, put the question this way: perhaps everything was not possible, "but can one say that everything that was possible was tried?"

One can reject this line of argument, but it remains true that Blum failed not merely to build upon the reforms of 1936 but even to protect them adequately against counterattack. He of course faced staggering difficulties: the antediluvian mentality of French industrialists, the doctrinaire rigidity of union leaders, the slavish commitment of the Communists to the policies of the Soviet Union, the tragic divisions resulting from the Spanish Civil War, the noisy rightist "patriotic" groups, the perverse individualism of nearly every Frenchman. Probably no political leader could have overcome the shortsighted selfishness and inertia or resisted the splintering factionalism that plagued French society in the late thirties.

Yet Blum's efforts were pitifully inadequate, no better or worse than those of the uninspired premiers who preceded and followed him.<sup>64</sup> Long

<sup>62</sup> The text is conveniently reprinted in Lefranc, *Histoire du Front Populaire*, 450-53. On the position of the Pivert faction and other leftist groups during the Blum regime, see Pierre Broué and Nicole Dorey, "Critiques de gauche et opposition révolutionnaire au Front Populaire (1936-1938)," *Le Mouvement Social*, 54 (1966): 91-133.

<sup>63</sup> Colton, *Blum*, 136-39; Léon Blum, *L'histoire jugera* (Paris, 1945), 266, 282; Daniel Guérin, *Front populaire révolution manquée: témoignage militant* (2d ed.; Paris, 1970), 111-18; Georges Lefranc, "Le courant planiste dans le mouvement ouvrier français de 1933 à 1936," *Le Mouvement Social*, 54 (1966): 89. Mendès France's remark was made in 1965 during a discussion of papers at a colloquium on Blum and his government. Fondation Nationale des Sciences Politiques, *Léon Blum*, 90.

<sup>64</sup> Tom Kemp, in a recent analysis of French economic problems in the 1930s, makes no distinction when he writes: "A prevailing crisis of confidence . . . paralyzed the decision-making powers of businessmen and politicians and made them . . . incapable of facing up to the depression." Tom Kemp, "The French Economy under the Franc Poincaré," *Economic History Review*, 24 (1971): 97-98.



years of balancing his socialist principles against his political ambitions and responsibilities had made him, in Joel Colton's words, a "tightrope-walker." Before the formation of the Popular Front he had, when forced to make a choice, always put his socialism above political accommodation. After he finally made the other choice in 1936, he became too much the politician, telling workers, despite the still-lagging French economy, that it was time for a "pause," that they must exercise "moderation and patience." It was not of central importance that, as the historian Alfred Sauvy has said, Blum's ignorance of economics was matched only by his sincerity. He could muster neither the flexibility of Roosevelt (whom he greatly admired) nor the ruthlessness of Hitler (whom he detested).<sup>65</sup>

During Blum's brief second term as premier, he revised his economic thinking. Aided by Georges Boris, author of an admiring study of the New Deal, *La Révolution Roosevelt* (1934), and one of the few Frenchmen familiar with the new Keynesian economics, Blum drafted a comprehensive program involving tax relief and government credits for defense industries, tax relief for the construction industry and small business, suspension of redemptions of the national debt, a special capital levy on the rich, a more progressive income tax, and rudimentary exchange controls. A massive common effort was necessary, he said, in order to build up French defenses, expand production, and maintain "social solidarity." The Assembly supported Blum's plan, but when the Senate voted it down he meekly resigned without even demanding the vote of confidence that might have compelled the Senate to yield. "To make the project succeed," Sauvy writes, "would have required a resounding appeal to all the forces of a country threatened with collapse. Unfortunately, Blum was not capable of such an effort."<sup>66</sup>

What he lacked was not courage but firmness, and the daring to step beyond the comfortable security of conventional political procedures. After the fall of his second government he confessed: "Perhaps, if I committed errors it was because of not having been enough of a leader."<sup>67</sup> None of this proves that a Blum like Roosevelt or—God forbid—Hitler could have provided France with the kind of *élan* that developed in the United States and Germany. It does, however, point up the psychological importance of Roosevelt and Hitler in their own countries.

<sup>65</sup> Colton, *Blum*, 89, 192; Sauvy, *Histoire économique*, 2: 303, 276–78. It is ironic that flexibility was the Rooseveltian quality that Blum admired most. On May 29, 1936, a few days before he assumed the premiership, Blum told Léon Jouhaux, head of the French labor unions: "What inspires me at the present moment is the example of Roosevelt . . . and especially his boldness, which has enabled him to change his methods when he realizes that they are not working out as planned." Quoted in Bernard Georges, "La C.G.T. et le gouvernement Léon Blum," *Le Mouvement Social*, 54 (1966): 55.

<sup>66</sup> Colton, *Blum*, 297–304; Georges Lefranc, *Le mouvement socialiste sous la troisième république: 1875–1910* (Paris, 1963), 352n; Sauvy, *Histoire économique*, 2: 277–78. See also, Tom Kemp, *The French Economy, 1919–1939: The History of a Decline* (London, 1972), 115–28. Kemp sees the failure of the Popular Front as essentially a political failure and writes of Blum's "lack of confidence in his own solution for the crisis" (p. 125).

<sup>67</sup> Colton, *Blum*, 281.

THESE PARALLELS suggest a number of generalizations about how the Great Depression influenced the United States and Germany. They do not, as I said at the start, indicate that the New Deal was a variant of fascism. The extraordinary expansion of the role of the federal government that took place in America cannot be equated with the Nazi totalitarian system, nor Hitler's despotism with the new executive power that Roosevelt exercised. The differences are qualitative not merely quantitative. But both governments experienced the depression as a tremendous crisis, and this fact shaped their responses in related ways. Furthermore the two regimes suffered from common intellectual, emotional, and organizational limitations that also led to analogous reactions to the depression.

Before Hitler and Roosevelt achieved power, more rigid and conservative leaders had tended to see the depression as a world-wide disease that would yield to international rather than national cures and indeed as one that governmental medicines could not alone eradicate. Hoover, despite his belief that European selfishness and shortsightedness had caused the depression, proposed his moratorium of 1931 "to give the forthcoming year to the economic recovery of the world," and Heinrich Brüning defended what he himself called his "draconian" emergency decrees as necessary to enable Germany "to meet its international obligations" and "conquer the economic crisis." Brüning, like Hoover, also believed that governmental policies of any kind could influence the economy relatively little. After the disastrous German elections of 1930, which made the Nazis a formidable political force, Brüning informed Hitler complacently: "According to our estimation, the crisis will last about four or five years more." And in a message on New Year's Day, 1931, he told the nation: "I am anxious to stress the limitations of any policy so that you will not indulge in any illusions."<sup>68</sup>

Nazis and New Dealers adopted more parochial but also more intense tactics, placing the economic well-being of their own societies ahead of world recovery and taking a far more optimistic view of what government could accomplish. While assuming the continuance of capitalism and in many ways adding to the wealth of private business groups, each nation sharply restricted the individual's freedom to pursue his economic interests and construed the power of government, and of executive power within the political system, in very broad terms. In addition, New Dealers and Nazis insisted that economic recovery could not be achieved without a certain amount of social restructuring and, furthermore, that society could be changed without exacerbating class conflicts. Indeed in both cases social reform was supposed to moderate such conflicts. But in both

<sup>68</sup> Hoover, *Great Depression*, 70; Kroll, *Weltwirtschaftskrise*, 364; Heinrich Brüning, *Memoiren: 1918-1934* (Stuttgart, 1970), 222-23, 192; Andreas Dorpalen, *Hindenburg and the Weimar Republic* (Princeton, 1964), 216. See also, Henning Köhler, "Arbeitsbeschaffung, Siedlung und Reparationen in der Schlussphase der Regierung Brüning," *Vierteljahrshefte für Zeitgeschichte*, 17 (1969): 304-06.

Roosevelt's America and Hitler's Germany economic and social objectives were subordinated whenever necessary to political aims.

That other nations adopted many of the tactics employed by New Dealers and Nazis scarcely needs demonstration. Depression policies everywhere were certainly based on national self-interest narrowly conceived, despite the obvious fact that the same plague was ravaging all. The tendency of governments to extend their sway in economic affairs and of leaders to be heavily influenced by political considerations was virtually universal. Whether conservative, moderate, or radical, few if any of the statesmen of the thirties remained indifferent to the suffering of their constituents or unwilling to sanction changes designed to alleviate it. The difference between the American and German depression experiences and those of other nations was in large measure psychological, resulting from Roosevelt's and Hitler's personal qualities of leadership and from their responses to the particular conditions in the two countries.

Again the comparison with Hoover and Brüning is at least suggestive: both lacked political tact and the ability to project an impression of warmth, sympathy, and self-assurance. In 1931 Walter Lippmann described Hoover as "indecisive and hesitant in dealing with political issues," and Arthur Krock commented on his "awkwardness of manner and speech and lack of mass magnetism." A recent student of the Hoover administration, Albert U. Romasco, remarks on his "inability to master the political techniques of leadership." As for Brüning, the historian Theodor Eschenburg, who knew him personally and considered him a "statesman of the highest intellectual gifts," admits that he had neither "the psychological talent" to win public backing nor "the tactical ability" to manage politicians. "He thought in terms of policy, not of human beings." And Andreas Dorpalen, another historian who lived through the Brüning period as a student in Germany, describes him as a "shy, withdrawn man [who] was unable to arouse the nation," a person lacking in warmth and imagination. Dorpalen's statement that the German public "mistook the chancellor's sober factualness for cynical coldness" could as well be applied to Hoover.<sup>69</sup>

So far as the depression is concerned, Roosevelt and Hitler, the one essentially benign, the other malevolent, justified far-reaching constitutional changes as being necessary to the improvement of economic conditions in a grave emergency but used change also as a device for mobilizing the psychic energies of the people. Yet both their administrations were plagued by infighting and confusion, partly because of genuine conflicts of interest and philosophy within the two diverse societies, but partly

<sup>69</sup> Walter Lippmann, *Interpretations: 1931-1932* (New York, 1932), 67; Albert U. Romasco, *The Poverty of Abundance: Hoover, the Nation, the Depression* (New York, 1965), 202, 213; Theodor Eschenburg, "The Role of Personality in the Crisis of the Weimar Republic: Hindenburg, Brüning, Groener, Schleicher," in Hajo Holborn, ed., *Republic to Reich: The Making of the Nazi Revolution* (New York, 1972), 49-50; Dorpalen, *Hindenburg*, 195, 216-17.

because of ignorance. No one really knew how to end the depression or even how best to serve the different interests the governments presumed to represent. Time after time major American and German policies produced results neither anticipated nor desired, some of them—the effect of New Deal farm policy on share croppers and of its public housing policy on racial segregation,<sup>70</sup> and that of Nazi rearmament on urban concentration, for example—directly contrary to the leaders' intentions.

Hitler papered over confusion, doubts, and rivalries with the *Führerprinzip*, unquestioning obedience to the leader, who was presumed to know what was best. Roosevelt, on the other hand, made a virtue of flexibility and experimentation. Both, however, masterfully disguised the inadequacies and internal disagreements in their entourages and to a remarkable extent succeeded in convincing ordinary citizens of their own personal wisdom and dedication.

The differences in the degree and intensity with which psychological pressures were applied by Nazis and New Dealers were so great as to become differences in kind—leaving aside the brute Nazi suppression not merely of those who resisted or disagreed, but of all who did not fit the insane Hitlerian conception of the proper order of things. The two movements nevertheless reacted to the Great Depression in similar ways, distinct from those of other industrial nations. Of the two the Nazis were the more successful in curing the economic ills of the 1930s. They reduced unemployment and stimulated industrial production faster than the Americans did and, considering their resources, handled their monetary and trade problems more successfully, certainly more imaginatively. This was partly because the Nazis employed deficit financing on a larger scale<sup>71</sup> and partly because their totalitarian system better lent itself to the mobilization of society, both by force and by persuasion. By 1936 the depression was substantially over in Germany, far from finished in the United States. However, neither regime solved the problem of maintaining prosperity without war. The German leaders wanted war and used the economy to make war possible. One result was “prosperity”: full employment, increased output, hectic economic expansion. The Americans lacked this motivation, but when war was forced upon them they took the same approach and achieved the same result.

<sup>70</sup> “The New Deal’s inclusion of Negroes in programs designed to relieve the special problems created by the Depression must be balanced against certain adverse side effects. . . . The housing projects encouraged residential segregation . . . and played a crucial role in spreading slum conditions.” Christopher G. Wye, “The New Deal and the Negro Community: Toward a Broader Conceptualization,” *Journal of American History*, 59 (1972): 621–22.

<sup>71</sup> Between 1933 and 1939 the German national debt increased from 12.9 billion marks to 42.7 billion, the American from \$22.5 billion to \$40.4 billion. Fischer, *Deutsche Wirtschaftspolitik*, 101; Erbe, *Wirtschaftspolitik*, 34–35; *Historical Statistics*, 720.

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## The Atomic Bomb and the Origins of the Cold War: U. S. Atomic-Energy Policy and Diplomacy, 1941-45

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MARTIN J. SHERWIN

DURING THE SECOND WORLD WAR the atomic bomb was seen and valued as a potential rather than an actual instrument of policy. Responsible officials believed that its impact on diplomacy had to await its development and, perhaps, even a demonstration of its power. As Henry L. Stimson, the secretary of war, observed in his memoirs: "The bomb as a merely probable weapon had seemed a weak reed on which to rely, but the bomb as a colossal reality was very different."<sup>1</sup> That policy makers considered this difference before Hiroshima has been well documented, but whether they based wartime diplomatic policies upon an anticipated successful demonstration of the bomb's power remains a source of controversy.<sup>2</sup> Two questions delineate the issues in this debate. First, did the development of the atomic bomb affect the way American policy makers conducted diplomacy with the Soviet Union? Second, did diplomatic considerations related to the Soviet Union influence the decision to use the atomic bomb against Japan?

These important questions relating the atomic bomb to American diplomacy, and ultimately to the origins of the cold war, have been addressed almost exclusively to the formulation of policy during the early months of the Truman administration. As a result, two anterior

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<sup>1</sup> Henry L. Stimson and McGeorge Bundy, *On Active Service in Peace and War* (New York, 1947), 637.

<sup>2</sup> Compare the evidence and interpretations in the following studies: Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam* (New York, 1965); Herbert Feis, *The Atomic Bomb and the End of World War II* (Princeton, 1966); Len Giovannitti and Fred Freed, *The Decision To Drop the Bomb* (New York, 1965); Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946: A History of the United States Atomic Energy Commission*, 1 (University Park, Pa., 1962); and Walter Smith Schoenberger, *Decision of Destiny* (Athens, Ohio, 1969).

questions of equal importance, questions with implications for those already posed, have been overlooked. Did diplomatic considerations related to Soviet postwar behavior influence the formulation of Roosevelt's atomic-energy policies? What effect did the atomic legacy Truman inherited have on the diplomatic and atomic-energy policies of his administration?

To comprehend the nature of the relationship between atomic-energy and diplomatic policies that developed during the war, the bomb must be seen as policy makers saw it before Hiroshima, as a weapon that might be used to control postwar diplomacy. For this task our present view is conceptually inadequate. After more than a quarter century of experience we understand, as wartime policy makers did not, the bomb's limitations as a diplomatic instrument. To appreciate the profound influence of the unchallenged wartime assumption about the bomb's impact on diplomacy we must recognize the postwar purposes for which policy makers and their advisers believed the bomb could be used. In this effort Churchill's expectations must be scrutinized as carefully as Roosevelt's, and scientists' ideas must be considered along with those of politicians. Truman's decision to use the atomic bomb against Japan must be evaluated in the light of Roosevelt's atomic legacy, and the problems of impending peace must be considered along with the exigencies of war. To isolate the basic atomic-energy policy alternatives that emerged during the war requires that we first ask whether alternatives were, in fact, recognized.

What emerges most clearly from a close examination of wartime formulation of atomic-energy policy is the conclusion that policy makers never seriously questioned the assumption that the atomic bomb should be used against Germany or Japan. From October 9, 1941, the time of the first meeting to organize the atomic-energy project, Stimson, Roosevelt, and other members of the "top policy group" conceived of the development of the atomic bomb as an essential part of the total war effort.<sup>3</sup> Though the suggestion to build the bomb was initially made by scientists who feared that Germany might develop the weapon first, those with political responsibility for prosecuting the war accepted the circumstances of the bomb's creation as sufficient justification for its use against any enemy.

Having nurtured this point of view during the war, Stimson charged those who later criticized the use of the bomb with two errors. First, these critics asked the wrong question: it was not whether surrender could have been obtained without using the bomb but whether a different diplomatic and military course from that followed by the Truman administration would have achieved an earlier surrender. Second, the

<sup>3</sup> Vannevar Bush to James B. Conant, Oct. 9, 1941, Atomic Energy Commission (hereafter AEC), doc. 17, AEC Archives, Washington; Schoenberger, *Decision*, *passim*.



basic assumption of these critics was false: the idea that American policy should have been based primarily on a desire not to employ the bomb seemed as “irresponsible” as a policy controlled by a positive desire to use it. The war, not the bomb, Stimson argued, had been the primary focus of his attention; as secretary of war his responsibilities permitted no alternative.<sup>4</sup>

Stimson’s own wartime diary nevertheless indicates that from 1941 on, the problems associated with the atomic bomb moved steadily closer to the center of his own and Roosevelt’s concerns. As the war progressed, the implications of the weapon’s development became diplomatic as well as military, postwar as well as wartime. Recognizing that a monopoly of the atomic bomb gave the United States a powerful new military advantage, Roosevelt and Stimson became increasingly anxious to convert it to diplomatic advantage. In December 1944 they spoke of using the “secret” of the atomic bomb as a means of obtaining a *quid pro quo* from the Soviet Union. But viewing the bomb as a potential instrument of diplomacy, they were not moved to formulate a concrete plan for carrying out this exchange before the bomb was used. The bomb had “this unique peculiarity,” Stimson noted several months later in his diary; “Success is 99% assured, yet only by the first actual war trial of the weapon can the actual certainty be fixed.”<sup>5</sup> Whether or not the specter of postwar Soviet ambitions created “a positive desire” to ascertain the bomb’s power, until that decision was executed “atomic diplomacy” remained an idea that never crystallized into policy.<sup>6</sup>

Although Roosevelt left no definitive statement assigning a postwar role to the atomic bomb, his expectations for its potential diplomatic value can be recalled from the existing record. An analysis of the policies he chose from among the alternatives he faced suggests that the potential diplomatic value of the bomb began to shape his atomic-energy policies as early as 1943. He may have been cautious about counting on the bomb as a reality during the war, but he nevertheless consistently chose policy alternatives that would promote the postwar diplomatic potential of the bomb if the predictions of scientists proved true. These policies were based on the assumption that the bomb could be used effectively to secure postwar diplomatic aims; and this assumption was carried over from the Roosevelt to the Truman administration.

Despite general agreement that the bomb would be an extraordinarily important diplomatic factor after the war, those closely associated with its

<sup>4</sup> Stimson and Bundy, *On Active Service*, 628–29.

<sup>5</sup> Stimson, diary, Dec. 31, 1944, Apr. 6–11, 1945, Henry L. Stimson Papers, Yale University Library.

<sup>6</sup> The term “atomic diplomacy” has often been used, but to my knowledge it has never been specifically defined. I understand it to mean either the overt diplomatic or military brandishing of atomic weapons for the purpose of securing foreign-policy objectives, or a covert diplomatic strategy based upon considerations related to atomic weapons. In this article “atomic diplomacy” refers exclusively to United States–Soviet relations.

development did not agree on how to use it most effectively as an instrument of diplomacy. Convinced that wartime atomic-energy policies would have postwar diplomatic consequences, several scientists advised Roosevelt to adopt policies aimed at achieving a postwar international control system. Churchill, on the other hand, urged the president to maintain the Anglo-American atomic monopoly as a diplomatic counter against the postwar ambitions of other nations—particularly against the Soviet Union. Roosevelt fashioned his atomic-energy policies from the choices he made between these conflicting recommendations. In 1943 he rejected the counsel of his science advisers and began to consider the diplomatic component of atomic-energy policy in consultation with Churchill alone. This decision-making procedure and Roosevelt's untimely death have left his motives ambiguous. Nevertheless it is clear that he pursued policies consistent with Churchill's monopolistic, anti-Soviet views.

The findings of this study thus raise serious questions concerning generalizations historians have commonly made about Roosevelt's diplomacy: that it was consistent with his public reputation for cooperation and conciliation; that he was naive with respect to postwar Soviet behavior; that, like Wilson, he believed in collective security as an effective guarantor of national safety; and that he made every possible effort to assure that the Soviet Union and its allies would continue to function as postwar partners.<sup>7</sup> Although this article does not dispute the view that Roosevelt desired amicable postwar relations with the Soviet Union, or even that he worked hard to achieve them, it does suggest that historians have exaggerated his confidence in (and perhaps his commitment to) such an outcome. His most secret and among his most important long-range decisions—those responsible for prescribing a diplomatic role for the atomic bomb—reflected his lack of confidence. Finally, in light of this study's conclusions, the widely held assumption that Truman's attitude toward the atomic bomb was substantially different from Roosevelt's must also be revised.

LIKE THE GRAND ALLIANCE itself, the Anglo-American atomic-energy partnership was forged by the war and its exigencies. The threat of a German atomic bomb precipitated a hasty marriage of convenience between British research and American resources. When scientists in Britain proposed a theory that explained how an atomic bomb might quickly be built, policy makers had to assume that German scientists were build-

<sup>7</sup> These views are represented in the following books and articles: Alperovitz, *Atomic Diplomacy*, 12–13; William Bullitt, "How We Won the War and Lost the Peace," *Life*, Aug. 30, 1948, pp. 82–97; Arthur Schlesinger, jr., "Origins of the Cold War," *Foreign Affairs*, 46 (1967): 26–29; and Herbert Feis, *Churchill, Roosevelt, Stalin: The War They Waged and the Peace They Sought* (Princeton, 1957), 596–98.

ing one.<sup>8</sup> "If such an explosive were made," Vannevar Bush, the director of the Office of Scientific Research and Development, told Roosevelt in July 1941, "it would be thousands of times more powerful than existing explosives, and its use might be determining." Roosevelt assumed nothing less. Even before the atomic-energy project was fully organized he assigned it the highest priority. He wanted the program "pushed not only in regard to development, but also with due regard to time. This is very much of the essence," he told Bush in March 1942. "We both felt painfully the dangers of doing nothing," Churchill recalled, referring to an early wartime discussion with Roosevelt about the bomb."

The high stakes at issue during the war did not prevent officials in Great Britain or the United States from considering the postwar implications of their atomic-energy decisions. As early as 1941, during the debate over whether to join the United States in an atomic-energy partnership, members of the British government's atomic-energy committee argued that the matter "was so important for the future that work should proceed in Britain."<sup>9</sup> Weighing the obvious difficulties of proceeding alone against the possible advantages of working with the United States, Sir John Anderson, then lord president of the council and the minister responsible for atomic-energy research, advocated the partnership. As he explained to Churchill, by working closely with the Americans British scientists would be able "to take up the work again [after the war], not where we left off, but where the combined effort had by then brought it."<sup>10</sup>

As early as October 1942 Roosevelt's science advisers exhibited a similar concern with the potential postwar value of atomic energy. After conducting a full-scale review of the atomic-energy project, James B. Conant, the president of Harvard University and Bush's deputy, recommended discontinuing the Anglo-American partnership "as far as development and manufacture is concerned." Conant had in mind three considerations when he suggested a more limited arrangement with the British: first, the project had been transferred from scientific to military control; second, the United States was doing almost all the developmental work; and third, security dictated "moving in a direction of holding much more closely the information about the development of this program." Under these conditions it was difficult, Conant observed, "to see how a joint British-American project could be sponsored in this

<sup>8</sup> The critical break-through was made by Otto R. Frisch and Rudolph E. Peierls in April 1940. For details of the British contribution, see Margaret Gowing, *Britain and Atomic Energy* (London, 1964), pt. 1, app. 1.

<sup>9</sup> Bush to Roosevelt, July 16, 1941; Roosevelt to Bush, Mar. 11, 1942, President's Secretary's File (hereafter PSF), Bush folder, Franklin D. Roosevelt Library (hereafter FDRL), Hyde Park, N.Y.; Winston S. Churchill, *The Second World War* (New York, 1962), 4: 330.

<sup>10</sup> This option was impractical due to scarce resources and the danger to project sites from German bombing. Quotation from Gowing, *Britain*, 73, 78.

<sup>11</sup> "Minute from Sir John Anderson to Prime Minister, 30.7.42," in *ibid.*, app. 3, pp. 437-38.

country.”<sup>12</sup> What prompted Conant’s recommendations, however, was his suspicion—soon to be shared by other senior atomic-energy administrators—that the British were rather more concerned with information for postwar industrial purposes than for wartime use.<sup>13</sup> What right did the British have to the fruits of American labor? “We were doing nine-tenths of the work,” Stimson told Roosevelt in October.<sup>14</sup> By December 1942 there was general agreement among the president’s atomic-energy advisers that the British no longer had a valid claim to all atomic-energy information.

Conant’s arguments and suggestions for a more limited partnership were incorporated into a “Report to the President by the Military Policy Committee.” Roosevelt approved the recommendations on December 28. Early in January the British were officially informed that the rules governing the Anglo-American atomic-energy partnership had been altered on “orders from the top.”<sup>15</sup>

By approving the policy of “restricted interchange” Roosevelt undermined a major incentive for British cooperation. It is not surprising, therefore, that Churchill took up the matter directly with the president and with Harry Hopkins, “Roosevelt’s own, personal Foreign Office.” The prime minister’s initial response to the new policy reflected his determination to have it reversed: “That we should each work separately,” he threatened, “would be a sombre decision.”<sup>16</sup>

Conant and Bush understood the implications of Churchill’s intervention and sought to counter its effect. “It is our duty,” Conant wrote Bush, “to see to it that the President of the United States, in writing, is informed of what is involved in these decisions.” Their memorandums no longer concentrated on tortuous discussions differentiating between the scientific research and the manufacturing stages of the bomb’s development but focused on what to Conant was “the major consideration . . . that of *national security and postwar strategic significance*.” Information on manufacturing an atomic bomb, Conant noted, was a “military secret which is in a totally different class from anything the world

<sup>12</sup> Conant to Bush, “Some thoughts concerning the S-1 project,” Oct. 26, 1942, AEC doc. 295.

<sup>13</sup> Conant to Bush, Mar. 25, 1943, Harry Hopkins Papers (hereafter HHP), A-Bomb folder, FDRL; Conant to Bush, “U.S.—British Relations on S-1 Project,” Nov. 13, 1942, AEC doc. 310; Leslie R. Groves, “Diplomatic History of the Manhattan Project” (hereafter “DHMP”), 7, 9, in Manhattan Engineer District Files (hereafter MED Files), National Archives. The Manhattan Engineer District, most commonly referred to as the Manhattan Project, was the cover name assigned by the United States Army to the atomic-energy project.

<sup>14</sup> Memorandum by Stimson, Oct. 29, 1942, in Groves, “DHMP,” annex 5.

<sup>15</sup> “Excerpt from Report to the President by the Military Policy Committee, Dec. 15, 1942, with Particular Reference to Recommendations Relating to Future Relations with the British and Canadians,” in *ibid.*, annex 6; Roosevelt to Bush, Dec. 28, 1942, PSF, Bush folder; Conant, “Memorandum on the interchange with the British and Canadians on S-1,” Jan. 7, 1943, AEC doc. 152.

<sup>16</sup> Robert E. Sherwood, *Roosevelt and Hopkins, An Intimate History* (New York, 1948), 202, 704; Churchill to Roosevelt, Apr. 1, 1943, HHP, A-Bomb folder.

has ever seen if the potentialities of this project are realized." To provide the British with detailed knowledge about the construction of a bomb "might be the equivalent to joint occupation of a fortress or strategic harbor in perpetuity."<sup>17</sup> Though British and American atomic-energy policies might coincide during the war, Conant and Bush expected them to conflict afterward.

The controversy over the policy of "restricted interchange" of atomic-energy information shifted attention to postwar diplomatic considerations. As Bush wrote to Hopkins, "We can hardly give away the fruits of our developments as a part of postwar planning except on the basis of some overall agreement on that subject, which agreement does not now exist."<sup>18</sup> The central issue was clearly drawn. The atomic-energy policy of the United States was related to the very fabric of Anglo-American postwar relations and, as Churchill would insist, to postwar relations between each of them and the Soviet Union. Just as the possibility of British postwar commercial competition had played a major role in shaping the U.S. policy of restricted interchange, the specter of Soviet postwar military power played a major role in shaping the prime minister's attitude toward atomic-energy policies in 1943.

"We cannot," Sir John Anderson wrote Churchill, "afford after the war to face the future without this weapon and rely entirely on America should Russia or some other power develop it."<sup>19</sup> The prime minister agreed. The atomic bomb was an instrument of postwar diplomacy that Britain had to have. He could cite numerous reasons for his determination to acquire an independent atomic arsenal after the war, but Great Britain's postwar military-diplomatic position with respect to the Soviet Union invariably led the list. When Bush and Stimson visited London in July, Churchill told them quite frankly that he was "vitally interested in the possession of all [atomic-energy] information because this will be necessary for Britain's independence in the future as well as for success during the war." Nor was Churchill evasive about his reasoning: "It would never do to have Germany or Russia win the race for something which might be used for international blackmail," he stated bluntly and then pointed out that "Russia might be in a position to accomplish this result unless we worked together."<sup>20</sup> In Washington, two months earlier, Churchill's science adviser Lord Cherwell had told Bush and Hopkins virtually the same thing. The British government, Cherwell stated, was considering "the whole [atomic-energy] affair on an after-the-war military basis." It intended, he said, "to manufacture and produce the weapon."<sup>21</sup>

<sup>17</sup> Conant to Bush, Mar. 23, 1943, HHP, A-Bomb folder. My italics.

<sup>18</sup> Bush to Hopkins, Mar. 31, 1943, *ibid.*

<sup>19</sup> Anderson, quoted in Gowing, *Britain*, 168.

<sup>20</sup> Harvey Bundy, "Memorandum of Meeting at 10 Downing Street on July 23, 1943," in Groves, "DHMP," annex 11; Bush to Conant, July 23, 1943, AEC doc. 312.

Prior to the convening of the Quebec Conference, Anderson explained his own and Churchill's view of the bomb to the Canadian prime minister, Mackenzie King. The British knew, Anderson said, "that both Germany and Russia were working on the same thing," which, he noted, "would be a terrific factor in the postwar world as giving an absolute control to whatever country possessed the secret."<sup>22</sup> Convinced that the British attitude toward the bomb would undermine any possibility of postwar cooperation with the Soviet Union, Bush and Conant vigorously continued to oppose any revival of the Anglo-American atomic-energy partnership.<sup>23</sup>

On July 20, however, Roosevelt chose to accept a recommendation from Hopkins to restore full partnership, and he ordered Bush to "renew, in an inclusive manner, the full exchange of information with the British."<sup>24</sup> A garbled trans-Atlantic cable to Bush reading "review" rather than "renew" gave him the opportunity to continue his negotiations in London with Churchill and thereby to modify the president's order.<sup>25</sup> But Bush could not alter Roosevelt's intentions. On August 19, at the Quebec Conference, the president and the prime minister agreed that the British would share the atomic bomb. Despite Bush's negotiations with Churchill, the Quebec Agreement revived the principle of an Anglo-American atomic-energy partnership, albeit the British were reinstated as junior rather than equal partners.<sup>26</sup>

The president's decision was not a casual one taken in ignorance. As the official history of the Atomic Energy Commission notes: "Both Roosevelt and Churchill knew that the stake of their diplomacy was a technological breakthrough so revolutionary that it transcended in importance even the bloody work of carrying the war to the heartland of the Nazi foe."<sup>27</sup> The president had been informed of Churchill's position as well as of Bush's and Conant's.<sup>28</sup> But how much closer Roosevelt was to Churchill than to his own advisers at this time is suggested by a report

<sup>21</sup> Bush, "Memorandum of Conference with Mr. Harry Hopkins and Lord Cherwell at the White House, May 25, 1943," HHP, A-Bomb folder; Bush, "Memorandum of Conference with the President," June 24, 1943, AEC doc. 133.

<sup>22</sup> Anderson, quoted in J. W. Pickersgill, *The Mackenzie King Record, 1939-1944*, 1 (Chicago, 1960): 532.

<sup>23</sup> Their arguments can be followed in Hewlett and Anderson, *New World*, 270-80; but see also Bush to Hopkins, Mar. 31, 1943, HHP, A-Bomb folder.

<sup>24</sup> Hopkins to Roosevelt, July 20, 1943; Roosevelt to Bush, July 20, 1943; Roosevelt to Churchill, July 20, 1943, HHP, A-Bomb folder.

<sup>25</sup> Bush to Bundy, July 27, 28, 1943, AEC docs. 313, 314; Bush, "Memorandum for the File: Sequence of events concerning interchange with the British on the subject of S-1," Aug. 4, 1943, AEC doc. 168; Hewlett and Anderson, *New World*, 227.

<sup>26</sup> Articles of Agreement Governing Collaboration between the Authorities of the U.S.A. and the U.K. in the Matter of Tube Alloys (hereafter Quebec Agreement), in Groves, "DHMP," annex 18, also in Gowing, *Britain*, app. 4. "Tube Alloys" was the British code name for the atomic-energy project.

<sup>27</sup> Hewlett and Anderson, *New World*, 280.

<sup>28</sup> Bush, "Memorandum for the File: Sequence of events concerning interchange with the British on the subject of S-1," Aug. 4, 1943, AEC doc. 168.



written after the war by General Leslie R. Groves, military director of the atomic-energy project. "It is not known what if any Americans President Roosevelt consulted at Quebec," Groves wrote. "It is doubtful if there were any. All that is known is that the Quebec Agreement was signed by President Roosevelt and that, as finally signed, it agreed practically in toto with the version presented by Sir John Anderson to Dr. Bush in Washington a few weeks earlier."<sup>29</sup>

The debate that preceded the Quebec Agreement is noteworthy for yet another reason: it led to a new relationship between Roosevelt and his atomic-energy advisers. After August 1943 the president did not consult with them about the diplomatic aspects of atomic-energy policy. Though he responded politely when they offered their views, he acted decisively only in consultation with Churchill. Bush and Conant appear to have lost a large measure of their influence because they had used it to oppose Churchill's position. What they did not suspect was the extent to which the president had come to share the prime minister's view.

It can be argued that Roosevelt, the political pragmatist, renewed the wartime atomic-energy partnership to keep relations with the British harmonious rather than disrupt them on the basis of a postwar issue. Indeed it seems logical that the president took this consideration into account. But it must also be recognized that he was perfectly comfortable with the concept Churchill advocated—that military power was a prerequisite to successful postwar diplomacy. As early as August 1941, during the Atlantic Conference, Roosevelt had rejected the idea that an "effective international organization" could be relied upon to keep the peace: an Anglo-American international police force would be far more effective, he told Churchill.<sup>30</sup> By the spring of 1942 the concept had broadened: the two "policemen" became four, and the idea was added that every other nation would be totally disarmed. "The Four Policemen" would have "to build up a reservoir of force so powerful that no aggressor would dare to challenge it," Roosevelt told Arthur Sweetser, an ardent internationalist. Violators first would be quarantined, and, if they persisted in their disruptive activities, bombed at the rate of a city a day until they agreed to behave. The president told Molotov about this idea in May, and in November he repeated it to Clark Eichelberger, who was coordinating the activities of the American internationalists. A year later, at the Teheran Conference, Roosevelt again discussed his idea, this time with Stalin. As Robert A. Divine has noted: "Roosevelt's concept of big power domination remained the central idea in his approach to international organization throughout World War II."<sup>31</sup>

<sup>29</sup> Groves, "DHMP," 15.

<sup>30</sup> *Foreign Relations of the United States* (hereafter *FRUS*), 1941, 1 (Washington, 1958): 363, 365–66.

<sup>31</sup> Roosevelt, quoted in "Mr. Sweetser's Notes," May 29, 1942, Arthur Sweetser Papers, box 39, Library of Congress (hereafter LC). This memorandum was brought to my attention by Mr.

Precisely how Roosevelt expected to integrate the atomic bomb into his plans for keeping the peace in the postwar world is not clear. However, against the background of his atomic-energy policy decisions of 1943 and his peace-keeping concepts, his actions in 1944 suggest that he intended to take full advantage of the bomb's potential as a postwar instrument of Anglo-American diplomacy. If Roosevelt thought the bomb could be used to create a more peaceful world order, he seems to have considered the threat of its power more effective than any opportunities it offered for international cooperation. If Roosevelt was less worried than Churchill about Soviet postwar ambitions, he was no less determined than the prime minister to avoid any commitments to the Soviets for the international control of atomic energy. There could still be four policemen, but only two of them would have the bomb.

THE ATOMIC-ENERGY POLICIES Roosevelt pursued during the remainder of his life reinforce this interpretation of his ideas for the postwar period. The following three questions offer a useful framework for analyzing his intentions. Did Roosevelt make any additional agreements with Churchill that would further support the view that he intended to maintain an Anglo-American monopoly after the war? Did Roosevelt demonstrate any interest in the international control of atomic energy? Was Roosevelt aware that an effort to maintain an Anglo-American monopoly of the atomic bomb might lead to a postwar atomic arms race with the Soviet Union?

An examination of the wartime activities of the eminent Danish physicist, Niels Bohr, who arrived in America early in 1944 as a consultant to the atomic-bomb project, will help answer these questions. "Officially and secretly he came to help the technical enterprise," noted J. Robert Oppenheimer, the director of the Los Alamos atomic-bomb laboratory, but "most secretly of all . . . he came to advance his case and his cause."<sup>32</sup> Bohr was convinced that a postwar atomic armaments race with the Soviet Union was inevitable unless Roosevelt and Churchill initiated efforts during the war to establish the international control of atomic energy.

Günter Brauch. For Roosevelt's remarks to Molotov, see *FRUS*, 1942, 3 (Washington, 1961): 573; to Eichelberger, see "President's Conversation at Luncheon with G.G.T. [Grace G. Tully] and S.I.R. [Samuel I. Rosenman], Nov. 13, 1942," in Elliott Roosevelt, ed., *F.D.R.: His Personal Letters, 1928-1945*, 4 (New York, 1950): 1366-67. See also *FRUS: The Conferences at Cairo and Teheran, 1943* (Washington, 1961), 530-32, and Robert A. Divine, *Roosevelt and World War II* (Baltimore, 1970), 58. And see Willard Range, *Franklin D. Roosevelt's World Order* (Athens, Ga., 1959), and Roland Stromberg, *Collective Security and American Foreign Policy: From the League of Nations to NATO* (New York, 1963).

<sup>32</sup> The details of Bohr's escape from Nazi-occupied Denmark can be followed in Gowing, *Britain*, 245-50; Oppenheimer's remarks can be found in his "Niels Bohr and Atomic Weapons," *New York Review of Books*, Dec. 17, 1966, p. 7. For additional information about Bohr's wartime activities, see also Liva Baker, *Felix Frankfurter* (New York, 1969), 271-80, and Ruth Moore, *Niels Bohr: The Man, His Science, and the World They Changed* (New York, 1966), 270-393.

Bohr's attempts to promote this idea in the United States were aided by Justice Felix Frankfurter.

Bohr and Frankfurter were old acquaintances. They had first met in 1933 at Oxford and then in 1939 on several occasions in London and the United States. At these meetings Bohr had been impressed by the breadth of Frankfurter's interests and, perhaps, overimpressed with his influence on Roosevelt. In 1944 the Danish minister to the United States brought them together, once again, at his home in Washington. Frankfurter, who appears to have suspected why Bohr had come to America and why this meeting had been arranged, had learned about the atomic-bomb project earlier in the war when, as he told the story, several troubled scientists had sought his advice on a matter of "greatest importance." He therefore invited Bohr to lunch in his chambers and, by dropping hints about his knowledge, encouraged Bohr to discuss the issue.<sup>33</sup>

After listening to Bohr's analysis of the postwar alternatives—an atomic armaments race or some form of international control—Frankfurter saw Roosevelt. Bohr had persuaded him, Frankfurter reported, that disastrous consequences would result if Russia learned on her own about the atomic-bomb project. Frankfurter suggested that it was a matter of great importance that the president explore the possibility of seeking an effective arrangement with the Soviets for controlling the bomb. He also noted that Bohr, whose knowledge of Soviet science was extensive, believed that the Russians had the capability to build their own atomic weapons. If the international control of atomic energy was not discussed among the Allies during the war, an atomic arms race between the Allies would almost certainly develop after the war. It seemed imperative, therefore, that Roosevelt consider approaching Stalin with a proposal as soon as possible.<sup>34</sup>

Frankfurter discussed these points with the president for an hour and a half, and he left feeling that Roosevelt was "plainly impressed by my account of the matter." When Frankfurter had suggested that the solution to this problem might be more important than all the plans for a world organization, Roosevelt had agreed. Moreover he had authorized Frankfurter to tell Bohr, who was scheduled to return to England, that he might inform "our friends in London that the President was most eager to explore the proper safeguards in relation to X [the atomic bomb]." Roosevelt also told Frankfurter that the problem of the atomic bomb "worried him to death" and that he was very eager for all the help he could have in dealing with it.<sup>35</sup>

<sup>33</sup> Frankfurter to Lord Halifax, Apr. 18, 1945, J. Robert Oppenheimer Papers (hereafter JROP), box 34, Frankfurter-Bohr folder, LC. I have been unable to discover the details surrounding Frankfurter's early informants.

<sup>34</sup> *Ibid.*; see also Gowing, *Britain*, 346–56.

<sup>35</sup> *Ibid.*

The alternatives placed before Roosevelt posed a difficult dilemma. On the one hand, he could continue to exclude the Soviet government from any official information about the development of the bomb, a policy that would probably strengthen America's postwar military-diplomatic position. But such a policy would also encourage Soviet mistrust of Anglo-American intentions and was bound to make postwar cooperation more difficult. On the other hand, Roosevelt could use the atomic-bomb project as an instrument of cooperation by informing Stalin of the American government's intention of cooperating in the development of a plan for the international control of atomic weapons, an objective that might never be achieved.

Either choice involved serious risks. Roosevelt had to balance the diplomatic advantages of being well ahead of the Soviet Union in atomic-energy production after the war against the advantages of initiating war-time negotiations for postwar cooperation. The issue here, it must be emphasized, is not whether the initiative Bohr suggested would have led to successful international control, but rather whether Roosevelt demonstrated any serious interest in laying the groundwork for such a policy.

Several considerations indicate that Roosevelt was already committed to a course of action that precluded Bohr's internationalist approach. First, Frankfurter appears to have been misled. Though Roosevelt's response had been characteristically agreeable, he did not mention Bohr's ideas to his atomic-energy advisers until September 1944, when he told Bush that he was very disturbed that Frankfurter had learned about the project.<sup>36</sup> Roosevelt knew at this time, moreover, that the Soviets were finding out on their own about the development of the atomic bomb. Security personnel had reported an active Communist cell in the Radiation Laboratory at the University of California. Their reports indicated that at least one scientist at Berkeley was selling information to Russian agents.<sup>37</sup> "They [Soviet agents] are already getting information about vital secrets and sending them to Russia," Stimson told the president on September 9, 1943. If Roosevelt was indeed worried to death about the effect the atomic bomb could have on Soviet-American postwar relations, he took no action to remove the potential danger, nor did he make any effort to explore the possibility of encouraging Soviet postwar cooperation on this problem. The available evidence indicates that he

<sup>36</sup> Bush, "Memorandum of Conference," Sept. 22, 1944, AEC doc. 185. Selected portions of this memorandum have been reprinted in *FRUS, The Conference at Quebec, 1944* (Washington, 1972), see especially 492n., but also see 296. Bush left this conference with the erroneous impression that Roosevelt had first become acquainted with Bohr's ideas several weeks earlier.

<sup>37</sup> See the testimony of Groves and of John Lansdale, Jr., in United States Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board* (Washington, 1954), 163-80, especially 171-74, and 258-81; Nucl Pharr Davis, *Lawrence and Oppenheimer* (New York, 1968), 191-92. Though the exact information being passed was not known, there can be no doubt that by the spring of 1944 Roosevelt was aware of Soviet interest in the Manhattan Project. See also Bohr to Roosevelt, July 3, 1944, JROP, box 34, Frankfurter-Bohr folder.

never discussed the merits of the international control of atomic energy with his advisers after this first or any subsequent meeting with Frankfurter.<sup>38</sup>

How is the president's policy of neither discussing international control nor promoting the idea to be explained if not by an intention to use the bomb as an instrument of Anglo-American postwar diplomacy? Perhaps his concern for maintaining the tightest possible secrecy against German espionage led him to oppose any discussion about the project. Or he may have concluded, after considering Bohr's analysis, that Soviet suspicion and mistrust would be further aroused if Stalin were informed of the existence of the project without receiving detailed information about the bomb's construction. The possibility also exists that Roosevelt believed that neither Congress nor the American public would approve of a policy giving the Soviet Union any measure of control over the new weapon. Finally Roosevelt might have thought that the spring of 1944 was not the proper moment for such an initiative.

Though it would be unreasonable to state categorically that these considerations did not contribute to his decision, they appear to have been secondary. Roosevelt was clearly, and properly, concerned about secrecy, but the most important secret with respect to Soviet-American relations was that the United States was developing an atomic bomb. And that secret, he was aware, already had been passed on to Moscow. Soviet mistrust of Anglo-American postwar intentions could only be exacerbated by continuing the existing policy. Moreover an attempt to initiate planning for international control of atomic energy would not have required the revelation of technical secrets. Nor is it sufficient to cite Roosevelt's well-known sensitivity to domestic politics as an explanation for his atomic-energy policies. He was willing to take enormous political risks, as he did at Yalta, to support his diplomatic objectives.<sup>39</sup>

Had Roosevelt avoided all postwar atomic-energy commitments, his lack of support for international control could have been interpreted as an attempt to reserve his opinion on the best course to follow. But he had made commitments in 1943 supporting Churchill's monopolistic, anti-Soviet position, and he continued to make others in 1944. On June 13, for example, Roosevelt and Churchill signed an Agreement and Declaration of Trust, specifying that the United States and Great Britain would cooperate in seeking to control available supplies of uranium and thorium ore both during and after the war.<sup>40</sup> This commitment, taken against the background of Roosevelt's peace-keeping ideas and his other

<sup>38</sup> Stimson, diary, Sept. 9, 1943. Frankfurter met several times with the president and discussed Bohr's proposal with him in great detail. See Max Freedman, ed., *Roosevelt and Frankfurter: Their Correspondence, 1928-1945* (Boston, 1967), 725.

<sup>39</sup> Diane Shaver Clemens, *Yalta* (New York, 1970).

<sup>40</sup> Agreement and Declaration of Trust, June 13, 1944, in Groves, "DHMP," annex 22a, also in Gowing, *Britain*, app. 7.

commitments, suggests that the president's attitude toward the international control of atomic energy was similar to the prime minister's.

Churchill had dismissed out of hand the concept of international control when Bohr talked with him about it in May 1944. Their meeting was not long under way before Churchill lost interest and became involved in an argument with Lord Cherwell, who was also present. Bohr, left out of the discussion, was frustrated and depressed; he was unable to return the conversation to what he considered the most important diplomatic problem of the war. When the allotted half hour elapsed, Bohr asked if he might send the prime minister a memorandum on the subject. A letter from Niels Bohr, Churchill bitinglly replied, was always welcome, but he hoped it would deal with a subject other than politics. As Bohr described their meeting: "We did not even speak the same language."<sup>41</sup>

Churchill rejected the assumption upon which Bohr's views were founded—that international control of atomic energy could be used as a cornerstone for constructing a peaceful world order. An atomic monopoly would be a significant diplomatic advantage in postwar diplomacy, and Churchill did not believe that anything useful could be gained by surrendering this advantage. The argument that a new weapon created a unique opportunity to refashion international affairs ignored every lesson Churchill read into history. "You can be quite sure," he would write in a memorandum less than a year later, "that any power that gets hold of the secret will try to make the article and this touches the existence of human society. This matter is out of all relation to anything else that exists in the world, and I could not think of participating in any disclosure to third or fourth parties at the present time."<sup>42</sup>

Several months after Bohr met Churchill, Frankfurter arranged a meeting between Bohr and Roosevelt. Their discussion lasted an hour and a half. Roosevelt told Bohr that contact with the Soviet Union along the lines he suggested had to be tried. The president also said he was optimistic that such an initiative would have a "good result." In his opinion Stalin was enough of a realist to understand the revolutionary importance of this development and its consequences. The president also expressed confidence that the prime minister would eventually share these views. They had disagreed in the past, he told Bohr, but they had always succeeded in resolving their differences.<sup>43</sup>

Roosevelt's enthusiasm for Bohr's ideas was more apparent than real. The president did not mention them to anyone until he met with Church-

<sup>41</sup> Bohr, quoted in Gowing, *Britain*, 355; Bohr to Churchill, May 22, 1944, JROP, box 34, Frankfurter-Bohr folder.

<sup>42</sup> Churchill, quoted in Gowing, *Britain*, 360.

<sup>43</sup> Aage Bohr, "The War Years and the Prospects Raised by the Atomic Weapons," in Stefan Rozenental, ed., *Niels Bohr* (New York, 1967), 206-07. In preparation for this interview, which took place on August 26, 1944, Bohr sent Roosevelt, through Frankfurter, a long memorandum: see Bohr to Frankfurter, July 5, 1944, JROP, box 34, Frankfurter-Bohr folder.



ill at Hyde Park on September 18, following the second wartime conference at Quebec. The decisions reached on atomic energy at Hyde Park were summarized and documented in an *aide-mémoire* signed by Roosevelt and Churchill on September 19, 1944. The agreement bears the markings of Churchill's attitude toward the atomic bomb and his poor opinion of Bohr. "Enquiries should be made," the last paragraph reads, "regarding the activities of Professor Bohr and steps taken to ensure that he is responsible for no leakage of information particularly to the Russians." If Bohr's activities prompted Roosevelt to suspect his loyalty, there can be no doubt that Churchill encouraged the president's suspicions. Atomic energy and Britain's future position as a world power had become part of a single equation for the prime minister. Bohr's ideas, like the earlier idea of restricted interchange, threatened the continuation of the Anglo-American atomic-energy partnership. With such great stakes at issue Churchill did not hesitate to discredit Bohr along with his ideas. "It seems to me," Churchill wrote to Cherwell soon after Hyde Park, "Bohr ought to be confined or at any rate made to see that he is very near the edge of mortal crimes."<sup>44</sup>

The *aide-mémoire* also contained an explicit rejection of any wartime efforts toward international control: "The suggestion that the world should be informed regarding tube alloys [the atomic bomb], with a view to an international agreement regarding its control and use, is not accepted. The matter should continue to be regarded as of the utmost secrecy."<sup>45</sup> But Bohr had never suggested that the world be informed about the atomic bomb. He had argued in memorandums and in person that peace was not possible unless the Soviet government—not the world—was officially notified only about the project's existence before the time when any discussion would appear coercive rather than friendly.

It was the second paragraph, however, that revealed the full extent of Roosevelt's agreement with Churchill's point of view. "Full collaboration between the United States and the British Government in developing tube alloys for military and commercial purposes," it noted, "should continue after the defeat of Japan unless and until terminated by joint agreement." Finally the *aide-mémoire* offers some insight into Roosevelt's intentions for the military use of the weapon in the war: "When a bomb is finally available, it might perhaps, after mature consideration, be used against the Japanese, who should be warned that this bombardment will be repeated until they surrender."<sup>46</sup>

Within the context of the complex problem of the origins of the cold war the Hyde Park meeting is far more important than historians of

<sup>44</sup> For the *aide-mémoire*, see Gowing, *Britain*, app. 8; for Churchill's communication to Cherwell, see page 358.

<sup>45</sup> *Ibid.*, 447.

<sup>46</sup> *Ibid.*

the war generally have recognized.<sup>47</sup> Overshadowed by the Second Quebec Conference on one side and by the drama of Yalta on the other, its significance often has been overlooked. But the agreements reached in September 1944 reflect a set of attitudes, aims, and assumptions that guided the relationship between the atomic bomb and American diplomacy during the Roosevelt administration and, through the transfer of its atomic legacy, during the Truman administration as well. Two alternatives had been recognized long before Roosevelt and Churchill met in 1944 at Hyde Park: the bomb could have been used to initiate a diplomatic effort to work out a system for its international control, or it could remain isolated during the war from any cooperative initiatives and held in reserve should cooperation fail. Roosevelt consistently favored the latter alternative. An insight into his reasoning is found in a memorandum Bush wrote following a conversation with Roosevelt several days after the Hyde Park meeting: "The President evidently thought he could join with Churchill in bringing about a US-UK postwar agreement on this subject [the atomic bomb] by which it would be held closely and presumably to control the peace of the world."<sup>48</sup> By 1944 Roosevelt's earlier musings about the four policemen had faded into the background. But the idea behind it, the concept of controlling the peace of the world by amassing overwhelming military power, appears to have remained a prominent feature of his postwar plans.

IN THE SEVEN MONTHS between his meeting with Churchill in September and his death the following April Roosevelt did not alter his atomic-energy policies. Nor did he reverse his earlier decision not to take his advisers into his confidence about diplomatic issues related to the new weapon. They were never told about the Hyde Park agreements, nor were they able to discuss with him their ideas for the postwar handling of atomic-energy affairs. Though officially uninformed, Bush suspected that Roosevelt had made a commitment to continue the atomic-energy partnership exclusively with the British after the war, and he, as well as Conant, opposed the idea. They believed such a policy "might well lead to extraordinary efforts on the part of Russia to establish its own position in the field secretly, and might lead to a clash, say 20 years from now."<sup>49</sup> Unable to reach the president directly, they sought to influence his policies through Stimson, whose access to Roosevelt's office (though not to his thoughts on atomic energy) was better than their own.

Summarizing their views on September 30 for the secretary of war,

<sup>47</sup> Herbert Feis mentions it in *The Atomic Bomb and the End of World War II* (Princeton, 1966), 33–34. He does not, however, draw out its full implications. See also John Lewis Gaddis, *The United States and the Origins of the Cold War, 1941–1947* (New York, 1972), 87.

<sup>48</sup> Bush to Conant, Sept. 25, 1944, AEC doc. 280.

<sup>49</sup> *Ibid.*

Bush and Conant predicted that an atomic bomb equivalent to from one to ten thousand tons of high explosive could be "demonstrated" before August 1, 1945. They doubted that the present American and British monopoly could be maintained for more than three or four years, and they pointed out that any nation with good technical and scientific resources could catch up; accidents of research, moreover, might even put some other nation ahead. In addition atomic bombs were only the first step along the road of nuclear weapons technology. In the not-too-distant future loomed the awesome prospect of a weapon perhaps a thousand times more destructive—the hydrogen bomb. Every major center of population in the world would then lie at the mercy of a nation that struck first in war. Security therefore could be found neither in secrecy nor even in the control of raw materials, for the supply of heavy hydrogen was practically unlimited.<sup>50</sup>

These predictions by Bush and Conant were more specific than Bohr's, but not dissimilar. They, too, believed that a nuclear arms race could be prevented only through international control. Their efforts were directed, however, toward abrogating existing agreements with the British rather than toward initiating new agreements with the Soviets. Like Bohr they based their hope for Stalin's eventual cooperation on his desire to avoid the circumstances that could lead to a nuclear war. But while Bohr urged Roosevelt to approach Stalin with the carrot of international control before the bomb became a reality, Bush and Conant were inclined to delay such an approach until the bomb was demonstrated, until it was clear that without international control the new weapon could be used as a terribly effective stick.

In their attempt to persuade Roosevelt to their point of view Bush and Conant failed. But their efforts were not in vain. By March 1945 Stimson shared their concerns, and he agreed that peace without international control was a forlorn hope. Postwar problems relating to the atomic bomb "went right down to the bottom facts of human nature, morals and government, and it is by far the most searching and important thing that I have had to do since I have been here in the office of Secretary of War," Stimson wrote on March 5. Ten days later he presented his views on postwar atomic-energy policy to Roosevelt.<sup>51</sup> This was their last meeting. In less than a month a new president took the oath of office.

Harry S. Truman inherited a set of military and diplomatic atomic-energy policies that included partially formulated intentions, several commitments to Churchill, and the assumption that the bomb would be a legitimate weapon to be used against Japan. But no policy was def-

<sup>50</sup> Bush and Conant to Stimson, Sept. 30, 1944, AEC doc. 281; Hewlett and Anderson, *New World*, 328–29.

<sup>51</sup> Stimson, diary, Mar. 5, 15, 1945.

initely settled. According to the Quebec Agreement the president had the option of deciding the future of the commercial aspects of the atomic-energy partnership according to his own estimate of what was fair.<sup>52</sup> Although the policy of "utmost secrecy" had been confirmed at Hyde Park the previous September, Roosevelt had not informed his atomic-energy advisers about the *aide-mémoire* he and Churchill signed. Although the assumption that the bomb would be used in the war was shared by those privy to its development, assumptions formulated early in the war were not necessarily valid at its conclusion. Yet Truman was bound to the past by his own uncertain position and by the prestige of his predecessor.<sup>53</sup> Since Roosevelt had refused to open negotiations with the Soviet government for the international control of atomic energy, and since he had never expressed any objection to the wartime use of the bomb, it would have required considerable political courage and confidence for Truman to alter those policies. Moreover it would have required the encouragement of his advisers, for under the circumstances the most serious constraint on the new president's choices was his dependence upon advice. So Truman's atomic legacy, while it included several options, did not necessarily entail complete freedom to choose from among all the possible alternatives.

"I think it is very important that I should have a talk with you as soon as possible on a highly secret matter," Stimson wrote to Truman on April 24. It has "such a bearing on our present foreign relations and has such an important effect upon all my thinking in this field that I think you ought to know about it without further delay."<sup>54</sup> Stimson had been preparing to brief Truman on the atomic bomb for almost ten days, but in the preceding twenty-four hours he had been seized by a sense of urgency. Relations with the Soviet Union had declined precipitously during the past week, the result, he thought, of the failure of the State Department to settle the major problems between the Allies before going ahead with the San Francisco Conference on the United Nations Organization. The secretary of state, Edward R. Stettinius, Jr., along with the department's Soviet specialists, now felt "compelled to bull the thing through." To get out of the "mess" they had created, Stimson wrote in his diary, they were urging Truman to get tough with the Russians.<sup>55</sup> He had. Twenty-four hours earlier the president met with the Soviet foreign minister, V. M. Molotov, and "with rather brutal frankness" accused his government of breaking the Yalta Agreement. Molotov was

<sup>52</sup> See point four of the Quebec Agreement, in Gowing, *Britain*, app. 4, p. 439.

<sup>53</sup> Kenneth Glazier, Jr., "The Decision to Use Atomic Weapons against Hiroshima and Nagasaki," *Public Policy*, 18 (1969): 463-516. Glazier emphasizes the bureaucratic momentum toward this decision.

<sup>54</sup> Stimson to Truman, Apr. 24, 1945, in Truman, *Memoirs*, vol. 1: *Year of Decisions* (Garden City, 1955), 85.

<sup>55</sup> Stimson, diary, Apr. 23, 1945.

furious. "I have never been talked to like that in my life," he told the president before leaving.<sup>56</sup>

With a memorandum on the "political aspects of the S-1 [atomic bomb's] performance" in hand and General Groves in reserve, Stimson went to the White House on April 25. The document he carried was the distillation of numerous decisions already taken, each one the product of attitudes that developed along with the new weapon. The secretary himself was not entirely aware of how various forces had shaped these decisions: the recommendations of Bush and Conant, the policies Roosevelt had followed, the uncertainties inherent in the wartime alliance, the oppressive concern for secrecy, and his own inclination to consider long-range implications. It was a curious document. Though its language revealed Stimson's sensitivity to the historic significance of the atomic bomb, he did not question the wisdom of using it against Japan. Nor did he suggest any concrete steps for developing a postwar policy. His objective was to inform Truman of the salient problems: the possibility of an atomic arms race, the danger of atomic war, and the necessity for international control if the United Nations Organization was to work. "If the problem of the proper use of this weapon can be solved," he wrote, "we would have the opportunity to bring the world into a pattern in which the peace of the world and our civilizations can be saved." To cope with this difficult challenge Stimson suggested the "establishment of a select committee" to consider the postwar problems inherent in the development of the bomb.<sup>57</sup> If his presentation was the "forceful statement" of the problem that historians of the Atomic Energy Commission have described it as being,<sup>58</sup> its force inhered in the problem itself, not in any bold formulations or initiatives he offered toward a solution. If, as another historian has claimed, this meeting led to a "strategy of delayed showdown," requiring "the delay of all disputes with Russia until the atomic bomb had been demonstrated,"<sup>59</sup> there is no evidence in the extant records of the meeting that Stimson had such a strategy in mind or that Truman misunderstood the secretary's views.

<sup>56</sup> Truman, *Year of Decisions*, 82; William D. Leahy, *I Was There* (New York, 1950), 351; Walter Millis, ed., *The Forrestal Diaries* (New York, 1951), 48-51.

<sup>57</sup> Stimson, "Memorandum discussed with the President," in his diary, Apr. 25, 1945.

<sup>58</sup> Hewlett and Anderson, *New World*, 343.

<sup>59</sup> Alperovitz, *Atomic Diplomacy*, 41-61, 270, *passim*. For sharply drawn essays by Alperovitz based on the themes and research in *Atomic Diplomacy*, see his *Cold War Essays* (New York, 1970), especially "How Did the Cold War Begin?" 35-50, and "The Use of the Atomic Bomb," 51-73. For essays that evaluate the contributions and deficiencies of *Atomic Diplomacy*, see Christopher Lasch, "The Cold War, Revisited and Re-Visioned," *New York Times Magazine*, Jan. 14, 1968, pp. 26-27, 44-59; Charles Maier, "Revisionism and the Interpretation of Cold War Origins," *Perspectives in American History*, 4 (1970): 313-47; Martin J. Sherwin, "The Atomic Bomb As History," *Wisconsin Magazine of History*, 53 (1969-70): 128-34; and Athan Theoharis, "Atomic Diplomacy," *New University Thought*, 5 (1967): 12, 73-77. For a recent, strained attack on Alperovitz's use of evidence, see Robert James Maddox, "Atomic Diplomacy: A Study in Creative Writing," *Journal of American History*, 59 (1973): 925-34; Alperovitz's response can be found on pages 1062-67.

What emerges from a careful reading of Stimson's diary, his memorandum of April 25 to Truman, a summary by Groves of the meeting, and Truman's recollections is an argument for overall caution in American diplomatic relations with the Soviet Union:<sup>60</sup> it was an argument against any showdown. Since the atomic bomb was potentially the most dangerous issue facing the postwar world and since the most desirable resolution of the problem was some form of international control, Soviet cooperation had to be secured. It was imprudent, Stimson suggested, to pursue a policy that would preclude the possibility of international cooperation on atomic-energy matters after the war ended. Truman's overall impression of Stimson's argument was that the secretary of war was "at least as much concerned with the role of the atomic bomb in the shaping of history as in its capacity to shorten the war."<sup>61</sup> These were indeed Stimson's dual concerns on April 25, and he could see no conflict between them.

Despite the profound consequences Stimson attributed to the development of the new weapon, he had not suggested that Truman reconsider its use against Japan. Nor had he thought to mention the possibility that chances of securing Soviet postwar cooperation might be diminished if Stalin did not receive a commitment to international control prior to an attack. The question of why these alternatives were overlooked naturally arises. Perhaps what Frankfurter once referred to as Stimson's habit of setting "his mind at one thing like the needle of an old victrola caught in a single groove" may help to explain his not mentioning these possibilities.<sup>62</sup> Yet Bush and Conant never raised them either. Even Niels Bohr had made a clear distinction between the bomb's wartime use and its postwar impact on diplomacy. "What role it [the atomic bomb] may play in the present war," Bohr had written to Roosevelt in July 1944, was a question "quite apart" from the overriding concern: the need to avoid an atomic arms race.<sup>63</sup>

The preoccupation with winning the war obviously helped to create this seeming dichotomy between the wartime use of the bomb and the potential postwar diplomatic problems with the Soviet Union raised by its development. But a closer look at how Bohr and Stimson each defined the nature of the diplomatic problem created by the bomb suggests that for the secretary of war and his advisers (and ultimately for the president they advised) there was no dichotomy at all. Bohr apprehended the meaning of the new weapon even before it was developed, and he had no doubt

<sup>60</sup> Stimson, diary, Apr. 25, 1945; Groves, "Report of Meeting with the President, Apr. 25, 1945," in Records of the Chief of Engineers, Commanding General's File 24, tab. D, MED Files; Truman, *Year of Decisions*, 87.

<sup>61</sup> Truman, *Year of Decisions*, 87.

<sup>62</sup> Frankfurter, quoted in Elting E. Morison, *Turmoil and Tradition: A Study of the Life and Times of Henry L. Stimson* (Boston, 1960), 167-68.

<sup>63</sup> Bohr to Roosevelt, July 3, 1944, JROP, box 34, Frankfurter-Bohr folder.



that scientists in the Soviet Union would also understand its profound implications for the postwar world. He was also certain that they would interpret the meaning of the development to Stalin just as scientists in the United States and Great Britain had explained it to Roosevelt and Churchill. Thus the diplomatic problem, as Bohr analyzed it, was not the need to convince Stalin that the atomic bomb was an unprecedented weapon that threatened the life of the world but the need to assure the Soviet leader that he had nothing to fear from the circumstances of its development. By informing Stalin during the war that the United States intended to cooperate with him in neutralizing the bomb through international control, Bohr reasoned that its wartime use could be considered apart from postwar problems.

Stimson approached the problem rather differently. Although he believed that the bomb "might even mean the doom of civilization or it might mean the perfection of civilization" he was less confident than Bohr that the weapon in an undeveloped state could be used as an effective instrument of diplomacy. Until its "actual certainty [was] fixed," Stimson considered any prior approach to Stalin as premature.<sup>64</sup> But as the uncertainties of impending peace became more apparent and worrisome, Stimson, Truman, and the secretary of state-designate, James F. Byrnes, began to think of the bomb as something of a diplomatic panacea for their postwar problems. Byrnes had told Truman in April that the bomb "might well put us in a position to dictate our own terms at the end of the war."<sup>65</sup> By June, Truman and Stimson were discussing "further *quid pro quos* which should be established in consideration for our taking them [the Soviet Union] into [atomic-energy] partnership." Assuming that the bomb's impact on diplomacy would be immediate and extraordinary, they agreed on no less than "the settlement of the Polish, Rumanian, Yugoslavian, and Manchurian problems." But they also concluded that no revelation would be made "to Russia or anyone else until the first bomb had been successfully laid on Japan."<sup>66</sup> Truman and Stimson based their expectations on how they saw and valued the bomb; its use against Japan, they reasoned, would transfer this view to the Soviet Union.

Was an implicit warning to Moscow, then, the principal reason for deciding to use the atomic bomb against Japan? In light of the ambiguity of the available evidence the question defies an unequivocal answer. What can be said with certainty is that Truman, Stimson, Byrnes, and several others involved in the decision consciously considered two effects of a combat demonstration of the bomb's power: first, the impact of the atomic attack on Japan's leaders, who might be persuaded thereby to end

<sup>64</sup> Stimson, diary, May 31, 1945.

<sup>65</sup> Byrnes, quoted in Truman, *Year of Decisions*, 87.

<sup>66</sup> Stimson, diary, June 6, 1945.

the war; and second, the impact of that attack on the Soviet Union's leaders, who might then prove to be more cooperative. But if the assumption that the bomb might bring the war to a rapid conclusion was the principal motive for using the atomic bomb, the expectation that its use would also inhibit Soviet diplomatic ambitions clearly discouraged any inclination to question that assumption.

Policy makers were not alone in expecting a military demonstration of the bomb to have a salubrious effect on international affairs. James Conant, for example, believed that such a demonstration would further the prospects for international control. "President Conant has written me," Stimson informed the news commentator Raymond Swing in February 1947, "that one of the principal reasons he had for advising me that the bomb must be used was that that was the only way to awaken the world to the necessity of abolishing war altogether." And the director of the atomic-energy laboratory at the University of Chicago made the same point to Stimson in June 1945: "If the bomb were not used in the present war," Arthur Compton noted, "the world would have no adequate warning as to what was to be expected if war should break out again." Even Edward Teller, who has publicly decried the attack on Hiroshima and declared his early opposition to it, adopted a similar position in July 1945. "Our only hope is in getting the facts of our results before the people," he wrote to his colleague, Leo Szilard, who was circulating a petition among scientists opposing the bomb's use. "This might help to convince everybody that the next war would be fatal," Teller noted. "For this purpose actual combat use might even be the best thing."<sup>67</sup>

Thus by the end of the war the most influential and widely accepted attitude toward the bomb was a logical extension of how the weapon was seen and valued earlier—as a potential instrument of diplomacy. Caught between the remnants of war and the uncertainties of peace, scientists as well as policy makers were trapped by the logic of their own unquestioned assumptions. By the summer of 1945 not only the conclusion of the war but the organization of an acceptable peace seemed to depend upon the success of the atomic attacks against Japan. When news of the successful atomic test of July 16 reached the president at the Potsdam Conference, he was visibly elated.<sup>68</sup> Stimson noted that Truman "was

<sup>67</sup> Stimson to Swing, Feb. 4, 1947, Stimson Papers; Arthur H. Compton, *Atomic Quest: A Personal Narrative* (New York, 1956), 236. The history of Szilard's petition is described in the following works: Leo Szilard, "Reminiscences," ed. Gertrude Weiss Szilard and Katherine R. Winsor, in Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration: Europe and America, 1930-1960* (Cambridge, Mass., 1969), 129-32; Alice K. Smith, "Behind the Decision to Use the Atomic Bomb: Chicago, 1944-45," *Bulletin of the Atomic Scientists*, Jan. 1957, pp. 303-05; Hewlett and Anderson, *New World*, 399-400; Teller to Szilard, July 2, 1945, JROP, box 71, Teller folder.

<sup>68</sup> Truman scheduled the Potsdam Conference to coincide with the test of the atomic bomb. See Stimson, diary, June 6, 1945; Joseph E. Davies, diary, May 21, 1945, Joseph E. Davies Papers, box 17, LC; Hewlett and Anderson, *New World*, 352; and Alperovitz, *Atomic Diplomacy*, 62-90.

tremendously pepped up by it and spoke to me of it again and again when I saw him. He said it gave him an entirely new feeling of confidence." The day after receiving the complete report of the test Truman altered his negotiating style. According to Churchill the president "got to the meeting after having read this report [and] he was a changed man. He told the Russians just where they got on and off and generally bossed the whole meeting."<sup>69</sup> After the plenary session on July 24 Truman "casually mentioned to Stalin" that the United States had "a new weapon of unusual destructive force."<sup>70</sup> Truman took this step in response to a recommendation by the Interim Committee, a group of political and scientific advisers organized by Stimson in May 1945 to advise the president on atomic-energy policy.<sup>71</sup> But it is an unavoidable conclusion that what the president told the premier followed the letter of the recommendation rather than its spirit, which embodied the hope that an overture to Stalin would initiate the process toward international control.<sup>72</sup> In less than three weeks the new weapon's destructive potential would be demonstrated to the world. Stalin would then be forced to reconsider his diplomatic goals. It is no wonder that upon learning of the raid against Hiroshima Truman exclaimed: "This is the greatest thing in history."<sup>73</sup>

As Stimson had expected, as a colossal reality the bomb was very different. But had American diplomacy been altered by it? Those who conducted diplomacy became more confident, more certain that through the accomplishments of American science, technology, and industry the "new world" could be made into one better than the old. But just how the atomic bomb would be used to help accomplish this ideal remained unclear. Three months and one day after Hiroshima was bombed Bush wrote that the whole matter of international relations on atomic energy "is in a thoroughly chaotic condition."<sup>74</sup> The wartime relationship between atomic-energy policy and diplomacy had been based upon the simple assumption that the Soviet government would surrender important geographical, political, and ideological objectives in exchange

<sup>69</sup> Stimson, diary, July 21, 1945; Churchill is quoted in *ibid.*, July 22, 1945.

<sup>70</sup> Truman, *Year of Decisions*, 416. Stalin knew that Truman was referring to the atomic bomb. See Georgii K. Zhukov, *The Memoirs of Marshal Zhukov*, tr. APN (New York, 1971), 674-75.

<sup>71</sup> The committee was composed of seven official members: Stimson served as chairman (in his absence his alternate was George L. Harrison); Ralph A. Bard, an undersecretary, represented the Navy Department; William L. Clayton, an assistant secretary, represented the State Department; Byrnes served as the president's personal representative; Bush, Conant, and Karl T. Compton, all scientist-administrators, completed the formal committee. In response to a suggestion by Conant the committee appointed a scientific panel composed of Arthur H. Compton, Enrico Fermi, Ernest O. Lawrence, and Oppenheimer.

<sup>72</sup> In addition to Truman's own description of his studied attempt to avoid any serious discussion with Stalin about the atomic bomb, see Charles E. Bohlen to Herbert Feis, Jan. 25, 1960, Herbert Feis Papers, box 14, I.C.

<sup>73</sup> Truman, *Year of Decisions*, 421.

<sup>74</sup> Stimson and Bundy, *On Active Service*, 637; Bush to Conant, Nov. 7, 1945, Vannevar Bush Papers, box 27, I.C.

for the neutralization of the new weapon. As a result of policies based on this assumption American diplomacy and prestige suffered grievously: an opportunity to gauge the Soviet Union's response during the war to the international control of atomic energy was missed, and an atomic-energy policy for dealing with the Soviet government after the war was ignored. Instead of promoting American postwar aims, wartime atomic-energy policies made them more difficult to achieve. As a group of scientists at the University of Chicago's atomic-energy laboratory presciently warned the government in June 1945: "It may be difficult to persuade the world that a nation which was capable of secretly preparing and suddenly releasing a weapon as indiscriminate as the [German] rocket bomb and a million times more destructive, is to be trusted in its proclaimed desire of having such weapons abolished by international agreement."<sup>75</sup> This reasoning, however, flowed from alternative assumptions formulated during the closing months of the war by scientists far removed from the wartime policy-making process. Hiroshima and Nagasaki, the culmination of that process, became the symbols of a new American barbarism, reinforcing charges, with dramatic circumstantial evidence, that the policies of the United States contributed to the origins of the cold war.<sup>76</sup>

<sup>75</sup> "The Franck Report," June 11, 1945, in Alice Kimball Smith, *A Peril and a Hope: The Atomic Scientists' Movement, 1945-1947* (Chicago, 1965), app. B.

<sup>76</sup> The charge was first made in the West by British physicist P. M. S. Blackett: "So we may conclude that the dropping of the atomic bomb was not so much the last military act of the second World War, as the first major operation of the cold diplomatic war with Russia now in progress." *Fear, War and the Bomb: Military and Political Consequences of Atomic Energy* (London, 1948), 139.

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## Franco Venturi on Russian Populism

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A Review Article by ALEXANDER GERSCHENKRON

*Veritatis cultor . . .*

FRANCO VENTURI. *Il populismo russo*. Volume 1, *Herzen, Bakunin, Černyševskij*; volume 2, *Dalla liberazione dei servi al nihilismo*; volume 3, *Dal'andata nel popolo al terrorismo*. (Piccola biblioteca Einaudi, 188–198) 2d ed.; Turin: Einaudi, 1972. Pp. cxii, 385; 479; 445.

FRANCO VENTURI. *Les intellectuels, le peuple et la révolution: Histoire du populisme russe au XIX<sup>e</sup> siècle*. Translated from the Italian by VIVIANA PAQUES. In two volumes. Paris: Gallimard, 1972. Pp. 667; 674–1166.

I BELIEVE that there are three main features by which a great book is recognizable: it must be interesting, memorable, and rereadable. These three criteria, which must be taken in conjunction—some books are rereadable only because they are promptly forgotten—apply most effectively to belles-lettres. But at times there are also remarkable scholarly books whose greatness can be judged by the same standards. Professor Franco Venturi's book on Russian populism is one of those rare specimens. The book first appeared two decades ago. At that time I read it carefully and reviewed it in the pages of this journal. Furthermore, stimulated by Venturi's study I elaborated the thoughts contained in my review in an essay.<sup>1</sup>

Thus I knew the book extremely well. Nevertheless when last year a second edition was published in Italy, followed immediately by a French translation of that edition, I reread the well-remembered, long pages with the same sense of intellectual excitement and esthetic pleasure I had felt during the first perusal. Indeed: interesting, memorable, and rereadable. But as is true in the case of any truly great book, there is infinitely more to Venturi's study than my perhaps not irrelevant but

<sup>1</sup> *AHR*, 59 (1953–54): 118–20; "The Problem of Economic Development in Russian Intellectual History," in E. J. Simmons, ed., *Continuity and Change in Russian and Soviet Thought* (Cambridge, Mass., 1955), 11–39, reprinted in Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1962), 152–87, together with an annex, "Realism and Utopia in Russian Economic Thought," 188–97.

rather superficial criteria. For in a very real sense the new edition is a new book, or rather it includes within the covers of volume 1 an Introduction (called *Présentation* in the French edition) that, with its more than one hundred pages, is to all intents and purposes an important book in itself. Moreover the new edition includes throughout the 1,300 pages of the text innumerable additions and corrections (*aggiunte e rettifiche*). But it is the Introduction that primarily justifies this attempt at a second appraisal.

The Introduction reveals with particular clarity the characteristics of the author. Franco Venturi is above all a truly indefatigable worker. His books appear regularly *a bruma ad brumam*, and *nullus annus sine libro* seems to be his device. The scope of his interests and the breadth and depth of his erudition are nothing if not amazing. In fact Russian intellectual history, to which he has made such a singular contribution, is but a side line of his efforts. He is first and foremost the historian of the Enlightenment. Before the appearance of his study on populism and in the years following it he published volumes and volumes on the French and Italian Enlightenment, culminating in the brilliant *Settecento riformatorio da Muratori a Beccaria* (1969).<sup>2</sup> This massive productivity did not prevent him from still continuing his researches on Russia, to which the engaging monograph *Il moto decabrista e i fratelli Poggi* (1956) bears witness. Along with his original work stands his immense editorial performance, in which nothing germane to the subject remains unread and unabsorbed. The magnificent critical edition of Cesare Beccaria (which appeared both in Italian and French) may serve as an example.<sup>3</sup> In the Russian field there is the splendid Italian edition of Alexander Radishchev's celebrated *Journey from Petersburg to Moscow* (1790), which just appeared in Italian.<sup>4</sup> Besides copious an-

<sup>2</sup> This volume of nearly 750 pages was adumbrated by numerous articles and is solidly based on the texts and the introductions contained in the huge volumes *Illuministi Italiani*, which Venturi published in the early sixties in the enormous *collana* called *La letteratura Italiana*.

<sup>3</sup> Cesare Beccaria, *Dei delitti e delle pene* [1764], ed. Franco Venturi (Turin, 1965). Venturi had previously published and introduced this text of Beccaria's along with other selections and letters from his pen in *Illuministi Italiani*, 3: 2-211. But the separate edition of Beccaria's classic is an entirely new work and a gem of editorial art. Preceded by a new introduction, the text is followed by nearly 600 pages of annotated letters and documents regarding the origins of Beccaria's book and the reactions to it in the following decade all over Europe from Spain to Russia.

<sup>4</sup> Alexandr N. Radišev, *Viaggio da Pietroburgo a Mosca*, ed. Franco Venturi (Bari, 1972). In editing this book in the midst of putting the new edition of *Russian Populism* through Italian and French presses Venturi enjoyed the cooperation of his wife, Signora Gigliola Venturi, who has made a name for herself as a talented translator of Russian prose and poetry. It might be added that the same year, 1972, also saw the appearance of Venturi's George Macaulay Trevelyan Lectures, *Utopia e riforma nell' illuminismo*, as well as the publication of a large volume of essays on the *settecento* entitled *Europe des lumières, Recherches sur le 18<sup>e</sup> siècle*. As this piece goes to press, Venturi has again surprised and delighted his readers by his enormous contribution (500 pages) to volume 3 of *Storia d'Italia, Dal primo Settecento all'Unità* (Turin, 1973). Venturi's study is called "L'Italia fuori d'Italia." He reviews in it a perfectly staggering amount of the period's literature on Italy in English, French, German, and Russian.



notations the edition contains a long and illuminating introduction from Venturi's pen, based not only on the study of the whole existing literature—beyond which Venturi, as is his wont, pushes on to a scrutiny of primary materials, including the little Masonic reviews that were appearing in Russia in the last quarters of the eighteenth century—but also on his incomparable knowledge of the *settecento*, which makes it possible for him to treat the Western roots of, and the foreign influences upon, this “first Russian intellectual” with a sureness of touch unequaled in previous writings on Radishchev, many of which are marred by Soviet chauvinism and dogmatism. The long years of Venturi's editorship of the *Rivista Storica Italiana*, surely one of the most outstanding scholarly historical periodicals, have been characterized not merely by wealth of ideas and organizational ability but, as is always true of the man, by sheer labor; in a recent issue of the *Rivista* (1972, no. 2) no less than one-third of the bulky number represents either original writings of Venturi's or translations done by him with unwavering attention to every detail down to the last footnote.

The introduction to the new edition of *Populismo russo* contains a survey of the literature on the subject, which appeared since the completion of the first edition. In Soviet Russia after Stalin's death it became possible to return to historical problems, the treatment of which had been stifled or simply forbidden during the lifetime of the despot. In the course of the two last decades Western historians have manifested a keen interest in Russian populism. Some of it was stimulated by Venturi's book, which was extensively reviewed both in the East and the West. Nothing of this large body of literature seems to have escaped Venturi's attention, and he applies to it his ability to summarize in a few paragraphs the crucial aspects of complex writings.<sup>5</sup> The Introduction thus contains a review of reviews of his book, a task to which Venturi applies the combination of firmness and grace that he revealed in his celebrated debate with the Soviet historian, N. M. Druzhinin.<sup>6</sup>

My own review of Venturi's book is also treated in the pages of the Introduction. In that review I expressed my admiration for the Herculean

<sup>5</sup> His summary in the Introduction of the momentous and very difficult magnum opus of Michael Confino, *Les systèmes agraires et progrès agricole* (Mouton, 1969)—the review of which has cost me months of study—bears witness to this extraordinary talent. See *Populismo russo*, 1: xlviii–lii; pp. 52–55 of the French edition henceforth F.E.).

<sup>6</sup> The debate included the following exchange: 1) Venturi, “L'autobiografia di uno storico Sovietico,” *Rivista Storica Italiana*, 74 (1962): 146–52; 2) Druzhinin, “Lettera aperta allo storico Italiano Franco Venturi”; 3) Venturi, “Risposta all'Accademico N. M. Druzhinin,” *Rivista Storica Italiana*, 75 (1963): 846–62; 4) Venturi, “Chiusura di un dibattito,” *ibid.*, 76 (1964): 1070–71; and 5) “Risposta dell'Accademico N. M. Druzhinin,” *ibid.*, 1072–85. Of the preceding items the following were also published in Soviet Russia: item 2: Druzhinin, “Otkrytoe pis'mo ital'yanskomu istoriku Franko Venturi,” *Istoriya S.S.S.R.*, 7 (1963): no. 4, pp. 182–83; item 3: Venturi, “Pis'mo akademiku N. M. Druzhininu”; and item 5: Druzhinin, “Otvét Franko Venturi,” *Istoriya S.S.S.R.*, 8 (1964): no. 5, pp. 189–203.

labor that in over three years of research in Moscow, in the libraries devastated and mutilated by Stalinist vandalism,<sup>7</sup> produced a study that, as I wrote then, "in its breadth and comprehensiveness is without precedent in the literature of the subject in any language." This judgment still holds today. Despite everything that has been written in twenty years, the absence of a precedent is still matched by the absence of any even remotely comparable "postcedent." But while I was able to recognize the great merits of the book, I also felt that some broad interpretation of Venturi's subject matter was in order. If this was criticism of the book, it was at best an oblique one. In those days I was in the process of developing my general approach to the industrial development of Europe by trying to understand the peculiarities of that development in the individual countries in terms of the level of their economic backwardness. In studying Venturi's book I was struck by how much in it—implicitly, and yet irresistibly—led to the insight that comprehension of the phenomenon of Russian populism required relating it to the economic and political backwardness of the country. The book seemed to me to merge with my own thinking; in fact the very selection and arrangement of the material as well as the distribution of emphasis by Venturi suggested that, even though he preferred not to indulge in explicit generalizations, his ideas and his approach were very close to my own. It is only now, after having read the Introduction and reread the book, that I have come to understand that I was, if not altogether wrong, certainly far from being fully right in this belief and that there is something quite fundamental about Venturi's approach that I failed to grasp.

But before I approach this crucial problem a related counter-criticism of my views by Venturi should be dealt with. I believed, and still believe, that it was the economic backwardness of Russia, its abysmal inferiority in comparison with the West, that forced the Russian intelligentsia to concern itself continually with the future destinies of the country. As I said in my review, the populists, in dealing with the problem, "clearly saw the advantages inherent in Russia's being a late-comer upon the modern historical scene" and "the possibility of adopting the results of foreign experience without incurring the heavy cost of experimentation." But they did so only in order to abandon the argument by an almost imperceptible twist and to raise the paradoxical claim that the preservation of the *old*—of the field commune (*obshchina*) and the workers' cooperatives (*artel*)—rather than the easy adoption of the *new* constituted the "advantages of backwardness." I regarded this course as

<sup>7</sup> It is only now that Venturi describes in the Introduction the grim physical conditions of research work in Moscow between 1947 and 1950. The difficulties of the intellectual readjustment cannot be overestimated. He went to Moscow to a new environment and an entirely new subject after publishing the first Italian edition of his book on the origins of the *Encyclopédie*, a second, improved and somewhat enriched, edition of which appeared in 1963. *Le origini dell'Enciclopedia* (Turin).

a "tragic surrender of realism to utopia" and saw therein one of the main reasons for the decline of populism in the last decades of the century in its contest with the specific Russian brand of Marxism. Speaking of realism and utopia in this connection, of course, must not be conceived in terms of the dogmatic dichotomy between the so-called utopian socialism and the self-styled Marxian scientific socialism.<sup>8</sup> The heart of the problem was an eminently empirical one. For the denial of the possibility of successful industrialization in Russia at a time when industrial development began to proceed at a very rapid rate was too conspicuously at variance with the facts.

I argued furthermore that it was also the political backwardness of the country which, along with economic backwardness, shaped the ideas and the actions of the populists. The existence of autocracy was bound to radicalize minds whose standards of political normalcy, whatever their rootedness in the native soil, invariably came from the West. Radicalism was the natural answer of an intelligentsia that was barred by absolutism from normal professional pursuits and confined to the area of pure thought, within which, despite the rigors of censorship, it was able to move with a degree of freedom that appears quite unbelievable by the standards of a modern totalitarian dictatorship. Hence populist thought wavered uneasily back and forth between radical anarchism and the equally radical apotheosis of an omnipotent Jacobin state. And when it came to action, any method of struggle seemed justified—from forged imperial manifestoes to murderous conspiracies against a government whose troops fired upon the peasants, flogged whole villages, and had jails, exile to forced labor in Siberia, and gallows in store for those who opposed it. Hence the complex and striking features of populists in whose minds and souls love for the people, devotion to the cause, heroism, and the spirit of sacrifice could be conjoined in some cases with the shattering dictum of a Dostoevski hero: *Vsyo pozvoleno* (Everything is permitted). And hence the unrelenting man hunt against the emperor, which ended with his assassination (March 1, 1881, Julian calendar) and the death on the gallows of the populist terrorists—the high drama with which Venturi still concludes his book.<sup>9</sup>

<sup>8</sup> It should be noted that, when in the late 1870s Russian socialists spoke of "scientific socialism," the reference was not at all specifically to Marxism but to the "entire Western corpus of socialist doctrine." See Abraham Asher, *Pavel Axelrod and the Development of Menshevism* (Cambridge, Mass., 1972), 48. Furthermore, it is a curiosity of Russian intellectual history that in that contest between the Marxists and the populists the latter, too, used Marx's analytical tenets just as much as the so-called Marxists and in fact in some respects were closer to Marxian ideas, as for instance in the insistence on the importance of a foreign market for capitalist development. Lenin vehemently denied the point with reference to Marxian schemas in volume 2 of *Das Kapital*, with the result that when, during World War I, he formulated in a popular pamphlet his views of imperialism, he had to crib his ideas from Hobson and Hilferding rather than from Marx and to spread a veil of silence over his previous theoretical position.

<sup>9</sup> There is little doubt that the history of Soviet dictatorship and the massive crimes committed by Stalin and his henchmen make it a good deal more difficult to see the horrors of Rus-

In his Introduction Venturi summarizes these views of mine with his usual skill and adds: "After having read those pages, the problem of populism is no longer what it used to be either for me or, I believe, for anyone else. Gerschenkron's interpretations remained a firm point in the discussions on the subject in the last fifteen years." This graceful acknowledgment must not be taken, however, to connote his readiness to accept my point of view without reservations. Rather he uses it as an occasion to formulate a general position regarding the very function in the historical process of ideas and political ideals. And he goes on to couch some propositions in the characteristically modest guise of questions:

An idea that appears to look backward in time, remolds itself on the past, seems to prefer what has been, and to eschew what will be—does really such an idea, whose function is destined to be negative, constitute a utopian retarding factor in economic and social development? Or does it not rather, at least at times, represent an act of *reculer pour mieux sauter*, that is to say, a fruitful attempt to preserve the most precious aspects of the past in order to transmit them to the future? History is not made just by looking forward but, I should say, by looking both forward and backward. Socialism is the idea of community and equality regarding material goods and of an economy based on solidarity. Is not socialism [so conceived] a legacy of the past that has been preserved by being transformed into an ideal for the future? And is it not true that socialism and communism, and not only populism, are ideas that in their origins were deliberately opposed to economic development?

In support of the oxymorons of these "assertive questions" Venturi quotes the title page of the "old and fundamental" book by Buonarrotti on the Babeuf conspiracy, a book through which, as he says, the communistic ideas of the eighteenth century were transmitted to the following century; and he regards the very motto of the book as "the most explicit declaration possible against economic development." The populists' idea of the field commune, their revolutionary will, and agrarian equality were things of the same order. According to Venturi these resemblances constitute "another reason for which populism should be seen as a page in the history of European socialism."<sup>10</sup>

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sian autocracy in the same light in which they were seen by the populist—and not only populist—intelligentsia in nineteenth-century Russia. In those days even the young Miliukov, who was to become a great historian and the leader of the Liberal party (*Kadety*), did not suppress a positive reaction to the regicide on the very day of the event. See the memoirs of P. N. Miliukov, *Vospominaniya* (New York, 1955), 1: 101. What if not the backwardness of the country was responsible for such attitudes, which so often were quite out of character for the individuals? A strong mental effort is necessary in order to place oneself within the climate of the time and to reject the obtrusive but fallacious thought that a government that did *not* exterminate millions and millions of its subjects was after all not so bad. One still cannot read Venturi's terse, accurate, and beautifully written account of the drama without high excitement, and still my feelings are no longer those with which I first learned the facts of the matter more than half a century ago. The knowledge that much more ruthless and vicious tyrants escaped the deserved vengeance intrudes perhaps irrelevantly but irresistibly and affects the sentiments of the reader.

<sup>10</sup> *Populismo russo*, 1: xxi–xxiii (F. E., 1: 27–29).

This spirited defense of populism goes far beyond the criticism of my approach. It is closely connected with Venturi's fundamental position as an intellectual historian, and more should be said about that later. At this point one may merely wonder whether Venturi's generalization does not go too far. In this connection I had better not mention Saint-Simon—the "utopian socialist"—who probably contributed more to the doctrines and the practice of economic development than any other single figure in the course of the nineteenth century. For Venturi in all likelihood would be quick to retort by casting doubt on both the adjective and the noun in the usual attributes applied to Saint-Simon. More pertinent would seem the fact that Venturi's emphasis appears to read out of socialism the basic acceptance of technological progress and economic development by Marx and the Marx-inspired socialist labor movements of the nineteenth century.<sup>11</sup> It is not difficult for me to accept Venturi's further somewhat implicitly stated opinion that, if the reconciliation between Russian intelligentsia and industrial development was carried out by Russian Marxism, the fact that such reconciliation could only occur under the auspices of a socialist ideology was, among other things, also the result of the strong intellectual tradition preestablished by at least a generation of populist thought.

On the other hand, I am more dubious when faced by Venturi's concluding—still interrogative—criticism of my views:

As far as concerns the internal history of populism in advancing gradually into the sixties and the seventies of the century, is it really possible to observe, as Gerschenkron would, a step backward, an outright reversal, an occlusion of the pre-

<sup>11</sup> I am not referring in this context to the history of Soviet Russia where Bolshevism has turned out in its practice to be the most enraged "economic developer" on historical record. For I interpret the policies of superindustrialization in the Soviet Union as flowing not from the theory or ideology of Marxism but from the needs of the dictatorship and the mechanics involved in the exercise of dictatorial power. With regard to the latter something might be said about the curious absorption by Bolshevism of some populist ideas. But that absorption was a highly selective one, and the most attractive traits of populism were certainly jettisoned in the process, so that not much could be made with regard to the problem at issue from a tenuous ideological connection that at best refers to the struggle for power rather than to the use made thereof in the postrevolutionary policies. Nevertheless, when everything is said and done, Venturi does have a point. He might have referred in corroboration of his view, among other things, to John Stuart Mill's extraordinarily friendly attitude toward the stationary state, which Mill's predecessors had regarded perhaps as inescapable but as very deplorable. There is little doubt that Mill's position was greatly influenced by the climate of contemporaneous socialist thought. On the other hand, however, it was from the same thought that there derived the assertion, repeated time and again in antisocialist literature, that under socialism the rate of investment in national income was bound to be fatally reduced. Much effort was spent by socialist writers in trying to controvert this assertion. Thus the problem is indeed a very complex one. It took generations of technological progress and enormous increases in standards of living in the industrialized countries for some small but vocal factions of the socialist thought of our days to return to the negative views on economic development, with the result that many a college student nowadays finds great fascination in reading about "technoclastic" episodes in the history of the labor movement, as they are detailed, for instance, by E. P. Thompson in his *The Making of the English Working Class* (London, 1965). But a "retropolation" from the specific conditions of the present to the whole body of socialist thought would be a rather unhistorical operation, and Venturi, I am sure, would be the first to scorn it.

viously possessed understanding of the advantages of backwardness, a relapse into a pure and simple worshipping of the past? Is *Narodnaya Volya* really in this respect a retrogression compared to Herzen's thought? Were not the *narodovol'tsy* the most conscious and most lucid judges of the Russian economic structure and of the fundamental role that the state and its measures always had played in the development of that structure? The debates of those years seem to testify to progress and not to retrogression. All these are things that Gerschenkron knows better than anyone else, but it should not be entirely useless to recall them in discussing with him the interpretation of Russian populism.<sup>12</sup>

Being acutely conscious of the limitations upon, and the manifold lacunae in, my knowledge of the subject, I cannot accept Venturi's complimentary remark. At the same time, however, I am also unable to accept his criticism as wholeheartedly as he would wish me to do. What certainly is correct is the fact that the populists toward the end of the debates of the seventies, quite apart from their contemporaneous opponents, understood the nature of the Russian state much more penetratingly than those Soviet scholars who still produce and reproduce in monotonous reiteration the elements of the basic Marxian theory of state. By contrast the populists said clearly that while "in Europe the state was a creation of the bourgeoisie, with us on the contrary bourgeoisie is being created by the state."<sup>13</sup> It is also true that the populists realized that they were witnessing "an extraordinarily important historical process—the process of the formation of the *tiers état*";<sup>14</sup> they refused to accept the inevitability of the process and to acquiesce in the impossibility of mitigating the negative aspects of the coming order, of changing it, or of limiting the duration of its dominance. For the state is not something "inert, impotent, and deprived of will."<sup>15</sup> This hope in the salutary intervention of the state was conjoined with the rather unelaborated, almost offhand assertion that industrial progress was a "necessity" for the state because of the growth of population and the pressure of international relations, which presumably meant the military needs.<sup>16</sup> It is also true that some changes in views took place over a very short time in the late seventies. In 1878 it was still asserted that in the West "history had placed the factory question in the center," while in Russia "that question was not advanced at all and was replaced by the agrarian question." Two or three years later this belief was somewhat weakened by the recognition that possibly not the villages but the cities, "because of higher development and greater mobility of city dwellers," may be "the first to raise the signal of rebellion."<sup>17</sup> All this can be readily admitted. And

<sup>12</sup> *Populismo russo*, 1: xxiv–xxv (F. E., 1: 30).

<sup>13</sup> See *Narodnicheskaya ekonomicheskaya literatura* (Moscow, 1958), 379; see also 377.

<sup>14</sup> *Ibid.*, 355.

<sup>15</sup> *Ibid.*, 356.

<sup>16</sup> *Ibid.*

<sup>17</sup> *Ibid.*, 323, 395.



yet as one rereads the relevant writings of the period, including the programs of the revolutionary parties then formed, certain things appear to stand out. They speak indeed of the revolution that will transfer all power to a constituent assembly in which the revolutionary party will stand for a democratically elected parliament, a complete bill of rights, land ownership by the people, and transfer of mills and factories to the workers. Thus the state is indeed accepted as the force to carry out the great social transformation, and the former anarchistic tendencies appear reduced to the well-tempered stress on decentralization and the autonomy of the communes of peasants and workers. But apart from the rather noncommittal reference to industrial progress just mentioned and apart from very occasional references to the benefits of introduction of labor-saving machinery, including technological progress on peasant farms, I am unable to find in this literature any reference to the chances of rapid increases of production and productivity—chances inherent precisely in the “advantages of backwardness” that earlier were understood and well-expressed, although immediately cast aside, both by Herzen and Chernyshevskii. Nor do I find any traces of real interest in, and understanding for, economic development, or any expectation that the state, be it the prerevolutionary or the postrevolutionary state, would or should promote economic development. The main concern of the state should be with the vast constitutional reform and with the creation of the collective structure of the economy. The interest in increasing output is hardly visible. Thus Venturi’s often-repeated and correct stress on the populists’ comprehension of the nature and importance of state power is rather irrelevant in the context of my approach to the problem of populism. In fact his reproach is not quite consistent with his previously stated belief in the basic antagonism of socialism to economic development. At any rate, on an empirical rather than ideological level the fatal underestimation by populist writers of the actual possibility and the promise of economic development in Russia of the eighties and nineties still remains an indubitable historical fact.

But I have come to realize that there is much more to the problem. For it seems to me now that any assessment of populism in the light of confrontation of populist thought with Russian economic and social history, however correct it may be in itself, ignores the impulses that caused Venturi to compose what he once called his two *troppo grossi volumi* as well as his basic purpose. He had set out to write a history of a great movement that excited both his admiration<sup>18</sup> and his intellectual curiosity by describing it such as it “really had been,” that is to say, by cleans-

<sup>18</sup> An admiration that did not make him close his eyes to the “absurdities” to which “some emotionally and intellectually less stable representatives of the movement were carried by the spirit of intransigence,” which Venturi sees as a distinct general characteristic of Russian history. *Populismo russo*, I: xv (F. E., I: 22).

ing it of the thick ideological crust and the manifold distortions under which it had been buried by much of the Soviet literature on the subject. It is only now in the Introduction to the new edition that the author's motivations and purpose are stated explicitly, and things become clear that could not be readily perceived by the reader of the first edition. This means that Venturi's discussion in the Introduction of the relevant Soviet writings, both old and new, must be seen as a crucial guide to the understanding of the book.

IN HIS DEBATE with Venturi, Druzhinin tried to lecture Venturi on the importance for the historian of having a "general conception of the world . . . a coherent view of the evolution of mankind." Without such a conception, Druzhinin argued, the historian can at best produce a talented narrative, and his work, reduced to a "naked description," loses all "cognitive value." Replying to the charge with dignity and courtesy, Venturi referred to the enormous costs of the adherence in Russia to a materialistic conception of history, conjoined, as it was, with Soviet nationalism and with the enforced neglect and ignorance of the developments outside the boundaries of the Soviet Union by the Soviet orthodoxy.<sup>19</sup>

As one rereads that debate and is illumined by the Introduction one realizes that nothing is farther from Venturi's mind than taking a general position on the role of generalization in history. Venturi is too considerable a historian not to know that according to Goethe's profound word "everything factual is already a theory."<sup>20</sup> For to the extent that a problem of "facts versus theory" exists to Venturi, it is altogether specific to the present case. The two *grossi* (but by no means *troppo grossi*) *volume* indeed contain a rejection of theory, but only in a very specific sense. The work reflects an aversion, if not revulsion, from what the Soviet version of dogmatic Marxism, guided by Stalin's illiterate literary ambitions and his orders, did to the treatment of a long, fascinating period of Russian intellectual and social history. During the years 1956-64 there were episodes or moments in Russia when it was as though scales had fallen from the eyes of Soviet scholars, and clear and sharp language was used in castigating the sins against historical scholarship that had been committed. At a conference, arranged by the Soviet Academy of Sciences in January 1964, F. B. Konstantinov, an Academician, expressed himself as follows on what the "cult of Stalin" had meant for "the science of history":

In this connection it is proper to speak of the main disabilities of certain historical works, which during the period of the cult of personality were written by Stalin himself and were inspired by him.

<sup>19</sup> See Venturi, "Risposta all'Accademico N. M. Druzhinin," 851, 861.

<sup>20</sup> Goethe, *Maximen und Reflexionen*, *Schriften zur Naturwissenschaft*, pt. 1 of *Sämtliche Werke*, Jubilaeumsausgabe, 39: 72.

Above all it was subjectivism—an arbitrary attitude to, and interpretation of, facts; the absence of historical veracity. If one is to talk about the demolition of confidence in our historical researches, the cause thereof lies in moving away from historical truth, in the subjective attitude to historical facts, in the anti-historical approach to phenomena. But what matters is not merely those researches in themselves, but the principles that were imposed upon scholars.

Konstantinov continued immediately with a concrete illustration of his indictment, an illustration that bears directly on the subject of Venturi's book. He said:

Let us take *the facts known to all* regarding the attitude to the heroic populists of the seventies. Let us recall how they were appraised by Marx, for instance in [the second Russian edition of] the preface to *The Communist Manifesto*, and by Lenin, both of whom very deservedly assigned to the populists a great role in the history of the revolutionary movement in Russia. And let us compare the evaluation of their role and participation in that movement given in [Stalin's] *Short Course*. It was such a misdirection of minds, such a misleading of views and appraisals, such a perverse interpretation of history that it defies imagination. That was an open revision of Marx's and Lenin's appraisals.<sup>21</sup>

"The facts known to all!" If Konstantinov could assert general knowledge of "the facts," the reason undoubtedly was that he spoke more than a decade after the appearance of Venturi's book, which in the interval had been so extensively reviewed in Russia. Even so, his reference to "all" would have been somewhat more convincing if Venturi's book had been translated into Russian—something that neither the favorable reviews nor Khrushchev's thaw had been able to induce.<sup>22</sup>

Konstantinov would have done well to refer to Venturi. Without such a reference it is quite unclear in what way the general knowledge of facts could have emerged from the perversions and prevarications that he described so angrily and so justly. Even so, the foregoing quotation described well the intellectual desert that Venturi found in Russia in those last

<sup>21</sup> Academy of Sciences of the USSR, *Istoriya i sotsiologiya* (History and Sociology) (Moscow, 1964), 91–92. My italics.

<sup>22</sup> It is worth quoting, however, what the previously mentioned Druzhinin said about the impact of Venturi's book in Soviet Russia. Speaking at a meeting of Soviet and Italian historians that took place in Moscow in October 1964, Druzhinin expressed himself as follows: "I should like also to pause here to say something about the significance of the works of our colleague Venturi. . . . Naturally, we are not fully in agreement with Venturi regarding the evaluation of populism and the characteristics of its origins. We start from different methodological principles and scrutinize differently the individual theories of the sixties and the seventies. But this cannot in the least obscure to us the great virtues of Professor Venturi's work on populism. This book had a fate of its own. Perhaps Professor Venturi does not know what I should like to stress: in the period when the cult of personality was being overcome, when the interest in the history of populism revived with us, when young people began studying that phenomenon, Venturi's book attracted enormous attention. For it contained a plethora of facts and was most interesting in its way of posing problems [*problematika*] in comparison with those antiquated and general surveys of the history of populism that had been published before the revolution and in the beginning of the Soviet period. This is indeed a broad canvas that had great historiographic importance." *Problemy sovetsko-ital'yanskoy istoriografii* (Problems of Italo-Soviet Historiography) (Moscow, 1966), 338.

years of the fifth decade of this century. It was that situation which aroused Venturi, the historian. The remedy for him was not to go back to the incontrovertible judgments of Marx and Lenin, the medicine suggested by Konstantinov, which reveals how deeply ingrained is the search for authorities even in a mind that has struggled valiantly to liberate itself from the authoritarian incubus. Venturi knows, of course, well what Marx said about the populists, and, the perfect editor that he is, he cannot forbear reproaching the Soviet editors for publishing Marx's correspondence with the Russian revolutionaries in Russian translation, only without the German, French, and English originals, as well as for publishing only one of the drafts of Marx's famous letter to Vera Zasulich.<sup>23</sup> And as far as Lenin was concerned, Venturi takes some pride in the fact that in the first edition of *Populismo russo* Lenin's name appears just once—in expressing the author's gratitude for assistance to the Lenin Library in Moscow. For the first thing to do in order to understand the populists, Venturi says, was "to leave Lenin alone." In fact Lenin's judgment of the populists is a theme that Venturi regards as a worthwhile but special subject. As the Introduction shows, Venturi is much more interested in the influence of populist thought upon Lenin.<sup>24</sup> Even so, for Venturi an essay in Marxology or Leninology could never be the answer to the problem. Rather the task was an immense cleansing job. What was needed was to study the record and to present it in detailed narration, in a display of texts and documents, as completely and as faithfully as possible and equipped with the full apparatus of references, and in the process to reveal the propelling force of ideas and the continuity of intellectual history as it impinged upon the Russian revolutionary movement, which for being "Russian socialism" was no less an integral part of European socialism. The task then was, in Venturi's words, "to oppose facts to theories, method to metaphysics, research to faith, the individual to the scheme,"<sup>25</sup> and, as Venturi may well have added, to oppose impartiality to bias, truth to lies, and, last but not least,

<sup>23</sup> *Populismo russo*, 1: xli (F. E., 1: 45).

<sup>24</sup> *Ibid.*, 1: ix (F. E., 1: 17). In Soviet Russia a serious discussion of the problem was impossible for decades beyond statements regarding Lenin's admiration for Chernyshevskii. Even in this respect it was left to an emigré writer to record Lenin's admission in a private conversation of the crucial nature of Chernyshevskii's impact upon young Lenin: he had "plowed me deeply all over [*vsogo perepakhal*]" was Lenin's reference to Chernyshevskii. See Nikolay Valentinov, *Vstrechy s Leninyim* (Encounters with Lenin) (New York, 1953), 103. The phrase appears correctly but colorlessly translated in Nikolay Valentinov (N. V. Volsky) *Encounters with Lenin*, tr. Paul Rosta and Brian Pearce (London, 1968), 64. It is only now that it is beginning to be realized in the Soviet Union that there was much more to Lenin's positive relation to the populists than the influence of Chernyshevskii, and a Soviet historian is able to trace Lenin's very gradual moving away from Plekhanov's traditional "dogmatic rejection of the populist perspective" and to emphasize the importance of the populist programs for the formation of Lenin's basic views. See M. Ya. Gefter, "Stranitsa iz istorii marksizma nachala XX veka" (A Page from the History of Marxism at the Beginning of the 20th Century), in *Istoricheskaya nauka i nekotorye problemy sovremennosti* (The Historical Science and Some Problems of Our Time) (Moscow, 1969), 24–25, 34.

<sup>25</sup> Venturi, "Risposta all'Accademico N. M. Druzhinin," 860.

objectivity to *partiynost'* (i.e., loyalty to the party as the all-dominating and all-overriding principle in scholarship and art), which was and still is Lenin's unholy legacy and which has caused so much perversion of both scholarship and art.<sup>26</sup> In these objectives and rejections lie the intellectual roots of Venturi's book, and they are indispensable for comprehending his purpose and for appraising and appreciating the result. In a true study of Russian populism empiricism was indeed what was called for. But the result stands high above crude or simple empiricism. For one thing, in scratching away the thick layers of dirt and revealing the huge canvas that lies hidden beneath them in all its multicolored glory, Venturi is intensely conscious of the unity of populism, that is to say, of the historical continuity that pervades and informs the whole painting. This relates, among other things, to the very concept of populism. Some years ago Professor Richard Pipes stressed the fact that the concepts of populism and populists entered the vocabulary only in the seventies in connection with the practical action of "going to the people." In the course of this movement the field commune and the workers' cooperative (the *obshchina* and the *artel'*) were encountered "unbookishly" in the real life of the peasants in the villages. It was only later in the following great debates that in Marxian writings the terms lost their specificity and were broadened far beyond their semantic origin.<sup>27</sup> Venturi does not deny—in fact he praises—the technical correctness of Pipes's findings. But when Pipes, drawing a large inference from those findings, claims that the widespread broad connotation of the term "populism" "has no historical justification," Venturi demurs, because the inference neglects the basic historical significance of the continuity in the history of ideas—the fact, that is, of the strong influence of the pre-

<sup>26</sup> As late as 1963 I. I. Mints, an Academician, still claimed in a scholarly discussion that "the principle of party loyalty [*partiynost'*] of scholarship" is the most important "principle of methodology of history." See Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 66. And at the same meeting the author of the main paper, P. N. Fedoseev, also an Academician, did not hesitate to express himself as follows: "What is the materialistic conception of history? According to our view, this is a party-loyal [*partiynoe*] conception of historical processes from the position of the working class, [that is] of the Marxist-Leninist party, and in our own conditions from the positions of the whole Soviet people, because the views of the party are shared by all Soviet men and women, by the whole Soviet people." *Ibid.*, 328. In the light of such a statement it is not surprising that the same author in his paper on "Methodological Problems of History" (written together with Yu. P. Fratsev, also an Academician) was able to say: "In determining the subject matter of historical science the guiding role belongs to the propositions of the Program of the Communist Party of the Soviet Union." In *ibid.*, 11. Just imagine Marc Bloch searching a political party program for authoritative guidance in his discussion of what *le métier d'historien* is or should be. And it is in the same light that one must read the statement by the same authors: "Nowadays, a Marxist historian departs from the propositions of historical materialism [regarding them] not as hypotheses, but as proven scientific propositions." *Ibid.*, 24. All this was being said at a meeting of scholars that forgathered in Moscow a decade after the death of Stalin and at which many a bitter word was spoken about what "the cult of personality" had done to the historical scholarship and the teaching of history in Soviet Russia. It is not difficult to imagine, proceeding *a minori ad maius*, what the situation was that Venturi found in Moscow in the years 1947–50.

<sup>27</sup> Richard Pipes, "Narodnichestvo [Populism]: A Semantic Inquiry," *Slavic Review*, 23 (1964): 441–58.

ceding intellectual development as contained in the writings of Herzen and Chernyshevskii in the fifties and the sixties upon the minds of those who "went to the people" in the seventies.<sup>28</sup> Venturi can take this position and thereby vindicate his own broad use of the concepts because from his painstaking empirical studies he has been able to draw an important generalization regarding the interdependence of thought and action in the development of what he so rightly subsumes under the general name of populism.<sup>29</sup>

It is certainly correct to say that Venturi is primarily interested in the history of ideas. But it would be quite wrong to say that he lacks interest in economic and social history. His preoccupation in the Introduction with the researches in agrarian economic history by Michael Confino testifies to his concerns in this direction. And so does his praise for the recent work (a series of books) of an able Soviet historian, P. A. Zayonchkovskii, who *inter alia* subjected the traditionally accepted statistics of peasant unrest to a searching and destructive criticism and in this connection also threw new light on the equally traditionally accepted thesis regarding the alleged "revolutionary situation" in Russia between 1859 and 1861; it was the same author who, destroying the received opinions on the "second revolutionary situation" in the late seventies, has also been able to demonstrate the extraordinary efficacy of the revolutionary populists and their impact upon the autocratic government.<sup>30</sup> In the same context it ought to be said that Venturi does not see the Soviet perversions of the history of populism as flowing exclusively from ideological dogmatism. He knows well that the exigencies of dictatorial politics—Stalin's fear of bombs and pistols—were at least as much responsible, and he mentions both Stalin's and Zhdanov's admission that concern with the populists might encourage terroristic activities against the Soviet government as well as difficulties in dealing with populism after what had been done in the collectivization to *narod*, to the people.<sup>31</sup>

Thus there is enormously more than empiricism to Venturi's study of Russian populism. The thought of the web of history is at all times strong upon him. It is not for nothing that he speaks of Stalin's violent cut through that web, and he always remains conscious that the object of his study was to lay bare *the roots of revolution*—the very appropriate title that his book received in the English translation.<sup>32</sup> It is in-

<sup>28</sup> *Populismo russo*, 1: xlv (F.E., 1: 49).

<sup>29</sup> One way of illustrating the point is to compare Nezhdanov, Turgenev's populist hero of the seventies, in his novel *Virgin Soil*, who is ready "to go to the people" and who first appears carrying books borrowed from the library, to Tolstoy's Pierre Bezukhov in *War and Peace*, who for a short time in 1812 becomes a disciple of the peasant Karataev and as such indeed a populist *ante litteram*, or rather *sine litteris*—something that surely cannot be said of the populists of the seventies.

<sup>30</sup> *Populismo russo*, 1: xlviii–lii, lxvii, xci (F.E., 1: 52–55, 70, 94).

<sup>31</sup> *Ibid.*, 1: xi (F.E., 1: 19).

<sup>32</sup> *Ibid.*



deed impossible to brush aside as a mere empiricist an author who in a dozen lines is able to present a compressed and profound explanation of the intellectual anatomy of Russian nihilism of the sixties.<sup>33</sup>

In dealing with the Soviet literature on populism Venturi is eminently fair. He lists the cases of serious historical scholarship and of debates among Soviet historians before the great Stalinist freeze. He welcomes with joy any sign of revival of historical scholarship after 1956 and is generous in his praise of what he calls a whole library on the subject that has been published within the last fifteen years, including particularly the large thirty-volume edition of Herzen's collected works as well as other significant reprints. But Venturi also sees clearly the other side. To his mind the deaths of Nicholas I and Stalin, the disappearance of the two despots who were the authors of tyranny and repression, are historical events that, if not comparable, are still worth comparison. It is a matter of deep regret to Venturi that the death of Stalin has not led to a clear "break, to a profound renewal" but merely to a "thaw" in the course of which much "old dirt and mud has been carried along" with the slowly melting surfaces.<sup>34</sup> As Venturi says:

The rediscovered men and facts press against the preestablished schemes, deforming and distorting them, so much that one would expect to see those schemes demolished at length. But the expectation may not be fulfilled. Is the monotonous repetition of ancient formulas in the prefaces and the conclusions of new research no more than an ideological ritual without substance?<sup>35</sup>

The ingrained habit of what in Soviet Russia is called *tsitatichnestvo* ("quotationism" in a literal translation) seems ineradicable.<sup>36</sup> The object of the "cult" has been replaced, and the works of Lenin perform the services previously performed by the works of Stalin. In one case even the ever-calm Venturi loses his equanimity. When a Soviet historian, N. N. Novikova, has meaningless recourse to a quotation from Lenin, Venturi breaks out: "And to think that she has done notable research even in private archives (which is a rare case in Soviet Russia)! But nothing stands, everything collapses before a general phrase, a journalistic mention, a simple, occasional illustration penned by Lenin."<sup>37</sup> And Venturi speaks of a "veritable obsession with Lenin."<sup>38</sup> The phrase could not be more precisely chosen. Serious scholars still do not think it beneath their dignity to support results of their thought and effort by quoting a

<sup>33</sup> *Ibid.*, 1: lxxvi (F.E., 1: 79).

<sup>34</sup> *Ibid.*, 1: xxvi (F.E., 1: 32).

<sup>35</sup> *Ibid.*, 1: lxxi (F.E., 1: 74).

<sup>36</sup> Also the term "citomania" is used. At the previously mentioned meeting the editor in chief of the main Soviet historical journal, *Voprosy istorii*, spoke sarcastically of the "faith in the infinite power of the quotation." Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 135.

<sup>37</sup> *Ma nulla vale e tutto crolla* . . . begins Venturi's outburst. *Populismo russo*, 1: lxx (F.E., 1: 73).

<sup>38</sup> *Una vera e propria ossessione leniniana. Ibid.*

dependent clause from Lenin's articles or speeches on historical subjects on which Lenin is known never to have performed any research.<sup>39</sup> On the other hand, the Soviet scholar is still to be discovered who would find anything, however trifling, to disagree with, however mildly, in the forty-five volumes (not counting the letters) and some 25,000 pages of the last edition of Lenin's complete works.<sup>40</sup> As long as this habit of adoration, whose roots reach back beyond the Stalinist epoch into the centuries of the history of the Byzantine Church, is not broken, one cannot expect to receive from Russia any historical work that in its scholarly purpose, objectivity, and the sense of the historian's duty and dignity would equal Venturi's study on Russian populism.

Venturi is not without optimism in this respect. He notes, as did some of the reviewers of his book (Sir Isaiah Berlin among them), that his book has found favorable reception both in the West and the East, and this, he says, precisely in the years of cold war. Has history then the power to override ideological and political conflicts? Venturi asks the question and says:

Despite everything the author of this book was and is convinced that an affirmative reply to this question was to be given twenty years ago and is to be given now. To be sure, the limits to Clio's capacity to clarify are obvious, but this does not mean that her labors are ineffective. Without being the remedy for all the evils, the historian's work still has served and will serve to arouse energies, to recreate the will of truth that can transcend any ideological and political barriers.<sup>41</sup>

One cannot read without being moved this article of faith of a great historian, for whom service of Clio means above all service of Truth, means being *veritatis cultor, fraudis inimicus*. And yet there is enough said in the Introduction to temper Venturi's optimism with regard to the future of the very subject of his book. There is too much in populism, as there is in the general history of socialism, that must go against the grain of the dictatorial system. And that is above all the problem of lib-

<sup>39</sup> For some striking, fairly recent examples, see Alexander Gerschenkron, "Soviet Marxism and Absolutism," *Slavic Review*, 30 (1971): 859-60.

<sup>40</sup> A Soviet historian has elaborated his view of the proper contents of Soviet works about Lenin: "If a scholarly study is devoted to the subject: 'Lenin about this or that historical phenomenon,' then the author obviously must embark upon research of the following questions: 'Why at a given time and in what connection Lenin turned to this phenomenon; what events in current social life had induced him to do so; what was the extent of literature and sources of the subject that was known to him; against whom did Lenin argue and why; what wrong propositions did he refute; what was the new that Lenin said compared to his predecessors as well as compared with his previous works; what was at the time the political significance of Lenin's pronouncements; what was—and what still is—the importance of these thoughts of Lenin's for the further development of our science and social practice' and so forth." Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 292. All this would be much better than the usual bouquets of quotations. In particular, the suggestion that Lenin's knowledge of sources may have been inadequate is certainly unusual, but one must still note the complete absence of any intimation that Lenin's views might have been all wrong and be subject to criticism.

<sup>41</sup> *Populismo russo*, 1: xxxiii-xxxiv (F.E., 1: 38-39).

erty. It is a great paradox—or perhaps a pseudodox—of Soviet intellectual history that Herzen has been traditionally accepted as “the great revolutionary democrat,” as goes the official designation repeated millions of times. It is indeed possible to honor Herzen by publishing the previously mentioned great edition of his works. But what is extremely difficult, if not impossible, is to study and to interpret Herzen’s work as it really was. In Venturi’s words in the Introduction:

The man of genius, Alexandr Ivanovich Herzen, organically resists any attempt to be forced into an academic party-dictated classification. Whosoever opens [the volumes of] the new accurate edition of his complete works cannot help but finding pallid and inane the explanations, so often repeated in Soviet literature about Herzen’s “liberal illusions.” Herzen is the man least suitable to become an icon or a portrait [in the gallery] of “socialist realism.” He raises continually, on every page, the problem of the relation between liberty and revolution.<sup>42</sup>

Effective and truthful study of Herzen as the great apostle of liberty requires, therefore, if not the solution of that problem, at least the liberation of the historians from the trammels of enforced schematisms and from the need to protect themselves by running fearfully behind the shelter of the irrelevant pronouncements of an allegedly omniscient demigod.

And there is still another and connected problem. In the concluding passage of his Introduction Venturi says that in Soviet Russia “now the sights are often directed to the experience of revolutionary populism because it is seen as a democratic experience, revealing the relationship between the masses of the people and the intelligentsia.” And he continues:

The inescapable problem, the point toward which this renewed interest leads is always the same: The confrontation between history and Marxism. . . . Personally, I am convinced that there is only one way out of this Marxian bind [*stretta*]: to understand that two centuries of socialist thought and movement in the whole of Europe are something much too variegated and much too rich to be monopolized by just one current thereof . . . and that any attempt to establish within the ambit of socialism one branch described as scientific, regarded as authentic, and opposed to all others described as utopian and fallacious, is not only historically erroneous, but will be conducive to a willful mutilation and distortion of the socialist thought in its entirety.<sup>43</sup>

The future will show whether this confrontation between Marxism and socialism will actually take place in Soviet Russia and, if it does, what the outcome will be. It seems to me that the problem as stated by Venturi is a specific Soviet problem, that is to say, much more a problem of Soviet Marxism, or of what goes under the name of Marxism in Soviet Russia, which is not to deny that in our days many a different and specious

<sup>42</sup> *Ibid.*, 1: lxxiii (F.E., 1: 76).

<sup>43</sup> *Ibid.*, 1: xcvi–xcviii (F.E., 1: 99).

interpretation equally misleadingly sails under the same name in the West. It remains, however, to be seen whether Venturi's exhortation—for this is what it is—will bear scholarly fruit in Russia, or whether the Russian historians in deference to their guardians will remain unmoved by Venturi's forceful language and will prefer to continue admiring his great contribution to the history of socialism from afar.

Thanks to the Introduction it is now possible to read Venturi's work with much more understanding of its meaning and message. It would be, therefore, extremely desirable to let the second edition appear in English, or at least to have the Introduction published as a separate book in an English translation.

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## On Diffusionism and Historicity

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A Review Article by R. C. PADDEN

CYRUS H. GORDON. *Before Columbus: Links between the Old World and Ancient America*. New York: Crown Publishers. 1971. Pp. xi, 15-224. \$6.50.

CARROLL L. RILEY *et al.*, editors. *Man across the Sea: Problems of Pre-Columbian Contacts*. Austin: University of Texas Press. 1971. Pp. xvii, 552. \$12.50.

IT WAS THE INSULARITY of the Western Hemisphere as established by Magellan that posed to Europeans of the sixteenth century the complex problem of American origins. Where did the Indians come from? When? How? What were their cultural derivations? Within the prevailing Augustinian world view the staggering dimensions of these questions could be made to shrink in proportion to movement back in time, for if one pushed the primary question back far enough in Christian reckoning one was bound to arrive at the Deluge and Noah. The answer then became obvious: by the historical assumptions of *Genesis* the newly discovered peoples of the Indies were held to have descended from the seed of Noah. This conclusion simultaneously laid the foundation for a doctrine of diffusionism that, in one guise or another, has been with us ever since.<sup>1</sup>

Modern diffusionists, of course, have long since found it necessary to quit scriptural exegesis if not their theories of interhemispherical diffusion. As part of the process of shucking off scriptural authority in favor of more scientific validation, men of the nineteenth century began to apply evolutionary theory to the study of culture. Broadly stated, this movement led to Adolf Bastian's classic postulation of a psychic unity of mankind wherein similar conditions of culture and environment tend to produce in man similar ideas that might also lead to analogous solutions of common problems. So postulated, by a process of parallelism there

<sup>1</sup> See Lee Eldridge Huddleston, *Origins of the American Indians* (Austin, 1967), for a discussion of and a guide to postdiscovery literature and Margaret T. Hodgen, *Early Anthropology in the Sixteenth and Seventeenth Centuries* (Philadelphia, 1964), especially chs. 6, 7, for a critical analysis of Biblical diffusionism and problems of cultural diversity. Also see Arnold Williams, *The Common Expositor: An Account of the Commentaries on Genesis, 1527-1633* (Chapel Hill, 1948).

was now a possibility of independent invention and isolated reinvention. Yet another omnipresent current of nineteenth-century thought combined with parallelism and evolutionary emphasis to produce a new vision of man held fast in the grip of inexorable Progress, rising from barbarism to civilization in readily perceptible stages. Up from savagery.<sup>2</sup> Unilinear cultural evolutionism, as the first major school in the new discipline of anthropology, thus posed a theoretical alternative to classic diffusionism. And, significantly, behind the postulates of cultural evolutionism lay a wealth of archeological and historical data for testing its theoretical constructs. Moving from the 1880s and Augustus Le Plongeon, that acidic and wrathful champion of Atlantean diffusion, to the 1920s and British anatomist G. Elliott Smith, whose Heliolithic theory of diffusion had all the world's high civilizations diffusing from the Nile valley, one encounters a spectrum of diffusionist theories, most of them leaning in the direction of archeological formulation, but without amassing convincing documentation to support their premises.<sup>3</sup> The loss of scriptural authority and failure to find validation for its tenets within a now-triumphant scientism gave to interhemispherical diffusionism both the status and vulnerability of an idea without a citadel.

More pressure was brought to bear on doctrinaire diffusionism with the coming of the Boas era in the early 1940s. In challenge to the sweeping generalizations of two new movements in anthropology—one of them neodiffusionist, the *Kulturkreis* school, the other racist—and to the rigidity of a now-sclerotic unilinear evolutionism, Franz Boas spearheaded a movement largely aimed at the implementation of historicity in archeology and anthropology. Rejecting the gratuitous formulation of airy theories incorporating grandiose generalizations of human behavior, the reformers demanded the substantiation of theory by hard evidence based upon research and field investigation.<sup>4</sup> One may read the agony of diffusionists in the basic assumptions that underlay most of their theoretical constructs in this period: Man is singularly uninventive; similarity of cultural traits from separated areas is an adequate basis upon which to postulate diffusion; and distance and lack of continuity in distribution are unimportant in comparing culture traits or complexes in separated areas. For the doctrinaire diffusionist it was the uninventiveness of man that ruled out the possibility of independent invention. In postulating interhemispherical diffusion on a basis of cultural similarity,

<sup>2</sup> Carroll L. Riley succinctly traces the rise and development of anthropology in the United States, in "American Historical Anthropology—An Appraisal," in Carroll L. Riley and Walter W. Taylor, eds., *Essays in Honor of Leslie Spier* (Carbondale, 1967), 3–21.

<sup>3</sup> See Robert Wauchoppe's informative and delightful *Lost Tribes and Sunken Continents: Myth and Method in the Study of American Indians* (Chicago, 1962); G. Elliott Smith, *The Diffusion of Culture* (London, 1933). A brief and devastating intellectual confrontation between Smith and three nondiffusionists was presented in G. Elliott Smith *et al.*, *Culture* (New York, 1927).

<sup>4</sup> Outlined in Riley, "American Historical Anthropology," 11–15.



likeness itself achieved the status of proof. Having thus postulated diffusion it followed that contact must have been made, the mere details of which could be relegated to a status of relative unimportance. How, one wonders parenthetically, could diffusionists have been expected to prove what were, judging by the preponderance of conflicting evidence against which they were held, articles of faith in the best Augustinian sense?

What amounted to salvation came to a whole generation of doctrinaire diffusionists in 1945 when A. L. Kroeber, a most distinguished alumnus of the Boas school, gave the Huxley Memorial Lecture. Laudably forsaking his customary vertical borings, he reported on a recent venture into the macrohistory of culture. He had expanded the ancient idea of *Oikoumenê*, that which the Greeks deemed the habitable world, to include the entire sweep of Old World civilization from Britain to Japan. Within this vast laboratory he traced and compared spatial ranges in successive waves of creativity through historical time; he noted the spread of sculpture of high esthetic value; he outlined shared cultural ideas and material components, like the bred horse, divinity of kingship, metallurgy, eunuchism, chess, divination, alchemy, and so on. Some recurrences were easily fathomed while others defied his means of inquiry. For the latter he postulated contextually analogous diffusion even though he had no direct supporting evidence. He recognized the existence of alternative explanations that involved parallelism or convergence but believed that the contextual suggestions of diffusion stemming from proximity and communication—in the presence of similar traits for which diffusion could be demonstrated—were strong enough to rule out autogenesis. And he found no evidence for the latter at all. Therefore, within the context of the *Oikoumenê* he proposed analogous diffusion as the most probable explanation where he lacked specific documentation. As a nursling of the Boas school Kroeber apparently felt obliged to justify his position and did so by stating a simple truism: as much evidence is needed for the assumption of independent invention as is needed to postulate diffusion. The burden of proof is equal. In the case at hand, on the strength of circumstantial evidence and without evidence to the contrary, he proposed the occurrence of analogous diffusion as a high probability.<sup>5</sup>

As so often happens in human affairs, what Kroeber originally said and meant has been eclipsed by what others have chosen to make of it. In one quantum leap interhemispherical diffusionists problematically joined the New World to Kroeber's *Oikoumenê*, as though his assumption of diffusion on a contextually analogous basis within the Old World *Oikoumenê* could by some miracle of analogical manipulation be made

<sup>5</sup> "The Ancient *Oikoumenê* as a Historic Culture Aggregate," in A. L. Kroeber, *The Nature of Culture* (Chicago, 1952): 379–95.

to prove theories of transoceanic diffusion for which both context and evidence were lacking. Ironically, Kroeber, whose whole methodological thrust rejected theoretical postulation without evidence, was now to be enlisted in its support. This necessitated certain omissions, the most important being Kroeber's studied exclusion of the New World from the *Oikoumenê* for lack of evidence. The conclusion that the New World had no place in the *Oikoumenê* was subsequently shared by Gordon W. Hewes, who, seeing Kroeber's *Oikoumenê* as a dynamic cultural process rather than a historical aggregate, converted it into a civilizational multiplier system.<sup>6</sup> Concentrating on processes, Mr. Hewes's analysis points up some of the kinds of evidence that would be apparent had there been prolonged interhemispherical contact before the Columbian era, but that are consistently lacking.

Fully separated from its contextual origins, Professor Kroeber's truism on the equality of the burden of proof emerged as the *dictum sanctorum* of anyone who would postulate interhemispherical diffusion. Where in the past diffusionists tended to be intimidated by lack of evidence, some now insisted that if they could not assume contact and diffusion without evidence, then traditionalists could not assume the isolation of the Western Hemisphere and its independent development, because the burden of proof was equal.<sup>7</sup> As was exemplified in a challenge made by a transpacific diffusionist, let the traditionalists prove that Aztec and Maya writing systems were invented without any suggestion from Old World systems. "In a historical question," responded Philip Phillips, "how does one prove that something did not happen otherwise than by pointing to the lack of evidence that it did?"<sup>8</sup>

The crux of the problem is that such a quantum leap in analogy fails to provide evidence of the intercommunication that is at once the engine of diffusion in Kroeber's *Oikoumenê* and the ultimate necessity of any process of transoceanic diffusion. Accordingly, diffusion and autogenesis would appear to be logical alternatives as explanations of the cultural origins of the New World: if there is no evidence of contact and diffusion then one must assume independent development because under such conditions probability lies inversely proportionate between them. The burden of ultimate proof remains equal, but interim assumptions of probability manifestly do not.

<sup>6</sup> Gordon W. Hewes, "The Ecumene as a Civilizational Multiplier System," in *Kroeber Anthropological Society Papers*, no. 25 (Berkeley, 1961), 73-109. This work has meaning for historians of culture.

<sup>7</sup> The equation is inherent in John L. Sorenson, "The Significance of an Apparent Relationship between the Ancient Near East and Mesoamerica," in Riley, *Man across the Sea*, 219-41, and Thor Heyerdahl, "An Introduction to Discussion of Transoceanic Contacts," in *Proceedings, XXXVII International Congress of Americanists* (Buenos Aires, 1968) 4: 67-88.

<sup>8</sup> Philip Phillips, "The Role of Transpacific Contacts in the Development of New World Pre-Columbian Civilizations," in *Handbook of Middle American Indians* (Austin, 1966), 4: 311-13. Cf. Robert Heine-Geldern, "The Problems of Transpacific Influences in Mesoamerica," *ibid.*, 277-95.

THE MOST RECENT enterprise in interhemispherical diffusionism is that undertaken by Cyrus H. Gordon in *Before Columbus: Links between the Old World and Ancient America*. According to Professor Gordon a highly sophisticated civilization of sea lords ruled and roamed the earth's seas in regular voyages of exploration and trade throughout the Bronze Age (ca. 3000–1200 B.C.). By their mastery of mathematical sciences and their cultural attainments they are held to have been the authors of all subsequent high civilizations. The Western Hemisphere was intimately known to them and under their aegis became the scene of a global intermingling of peoples and cultures. Corollary processes of diffusion created a number of brilliant American civilizations that, in their later phases, are known to us as Maya, Aztec, and Inca. Mr. Gordon concludes: "The breathtaking achievements of the Mesoamericans could not be, and were not, the works of savages who lifted themselves up by their bootstraps. Instead they are the culminations of mingled strands of civilization brought to these shores by a variety of talented people from Europe, Africa, and Asia." It seems essential that the author of so startling a thesis should be permitted to explain how it is developed and sustained:

Our method is straightforward. We shall follow the primary sources to their simple and direct logical conclusions, unhampered by opinion that runs counter to the primary sources. For this reason we will not be concerned with any Gallup Poll type of approach; 49 percent support does not make anything necessarily wrong; nor does 51 percent support make it necessarily right. . . . Nor are we committed to any school of thought such as "diffusionism" on the one hand or "independent inventionism" on the other.<sup>9</sup>

In the face of Professor Gordon's thesis, the latter disclaimer seems inexplicable.

As a proponent of cultural unitarianism Gordon believes that the real world-history of mankind has yet to be written, which suggests a philosophical ground commonly held by diffusionists: the universality of world history, as established by German historiography of the nineteenth century, was deformed by American pragmatism and its applied evolutionism and parallelism. But diffusionism is to revive universality through its historical unification of Europe, Asia, Africa, and Mesoamerica in the pre-Columbian era.<sup>10</sup> The affinity of such a view to Kroeber's *Oikoumenê* is obvious, and one finds Professor Gordon employing it simply as "ecumene" but without reference to those elements embarrassing to his thesis.

For a solution to the problematical lack of intercommunication and transport between the Old and New Worlds, the author avails himself

<sup>9</sup> Gordon, *Before Columbus*, 30, 16.

<sup>10</sup> See, for example, Wilhelm Koppers, "Das Problem der Universalgeschichte im Lichte von Ethnologie und Prä-historie," *Anthropos*, 52 (1957): 369–89.

of Charles H. Hapgood's *Maps of the Ancient Sea Kings*.<sup>11</sup> Working from the 1513 map of the Turkish admiral Piri Reis, Mr. Hapgood theorizes that the makers of the source maps—supposedly Bronze Age—from which the Piri Reis map was drawn had mastered the spherical trigonometry of map projection. If his analysis and extrapolations should be proved correct and if it could be established that the source maps were adequately ancient, then there would exist circumstantial evidence of a preclassical science and civilization that dwarfed every other until the nineteenth century. Mr. Hapgood's theories and some of their implications are incorporated in the Gordon thesis, not for consideration as theory but as a basis for assuming interhemispherical diffusion spanning the Bronze Age under the aegis of a preclassical thalassocracy.

There is an immediate problem: the chronologies of Mesoamerica's varied cultural precipitations, as they are presently understood through historical and archeological evidence, do not coincide with the Hapgood chronology and therein offer Mr. Gordon's thesis formidable contradiction. Moreover, if one is to predicate diffusion it is incumbent upon one to nucleate the evidence, to demonstrate the spatial and chronological overlaps between donor and recipient cultures within which specific traits of culture and their developmental chronologies are to be established. Professor Gordon makes no attempt to reconcile the chronological discrepancies, nor does he provide any sort of datum chronology. Instead he questions the validity of the evidence against his thesis by arguing in piecemeal fashion that archeology tends to confirm whatever is traditional. This is further explained as a result of overspecialization, which creates hyperskepticism, so that archeologists and historical traditionalists are prone to be hyperskeptical and negative toward new ideas.<sup>12</sup> As evidence he cites Heinrich Schliemann's dramatic unearthing of Troy, so long believed by archeologists to be nothing more than a figment of Homeric myth. This theme is sufficiently recurrent to suggest that Professor Gordon's ultimate purpose is to play Schliemann to Mesoamerican myth.

Having assumed the means of transoceanic diffusion in the Bronze Age, the author suggests a series of interhemispherical "links" that is held out as proof of his thesis. The master link in the chain, one forming a leitmotiv by its reinforcement of unrelated conclusions, is based on the imposing figure of Quetzalcóatl, the Plumed Serpent, whom the author invests with a specific myth. It is Professor Gordon's contention that Aztec, Maya, and Inca civilizations all shared an identical tradition wherein the civilized arts were originally brought to them by a bearded white man who came from the east over the Atlantic by boat. To the Aztecs he was Quetzalcóatl; the Maya called him Kukulcan; among the Inca he was known as Uiracocha. This Plumed Serpent myth, the author

<sup>11</sup> Charles H. Hapgood, *Maps of the Ancient Sea Kings* (Philadelphia, 1966).

<sup>12</sup> Gordon, *Before Columbus*, 170, 121, 79.

insists, was a consistent and persistent American tradition. On the other side of the Atlantic Professor Gordon points to a pediment from a temple on the Athenian Acropolis that portrays several bearded man-serpents, presumably out of Greek mythology, in feather-trimmed garb, and argues that the legends of Quetzalcóatl, like Greek legends, reflect "historic movements." Now the ghost of Schliemann appears via the analogical implication that overspecialized archeologists and ethnohistorians have missed the real significance of Quetzalcóatl, which is presently to be disclosed by an amateur armed with new ideas. Mr. Gordon forthwith identifies the Plumed Serpent as a meandering Mediterranean and postulates the link thus:

This embodies the essential traits—at two levels—of the American iconography. First, we are dealing with a bearded white man from the Mediterranean; second, he is at the same time a feathered serpent. There are too many details involved to be attributed to accident. The diffusion of ideas from the Mediterranean to Mesoamerica explains the facts more reasonably than a psychological approach implying that it is so natural for men to conceive of bearded white men who are at the same time feathered serpents, that the same combination naturally developed independently at ends of the earth in isolation.<sup>13</sup>

The validity of this link bears upon the historicity of Professor Gordon's entire thesis. Leaving aside his gratuitous assumption of transport and the unresolved chronological contradictions, one must raise a crucial question: Was there, in fact, such a persistent and consistent American Plumed Serpent tradition? Since the sixteenth century, dilettantes, whose writings on this theme usually gravitate to the "esoteric-occult-curiosa" stalls of used-bookshops, have suggested this refrain in part if not in whole.<sup>14</sup> But the primary sources deny it. It all depends upon whom one chooses to believe. Mesoamerican primary sources represent Quetzalcóatl as an integral part of the cultural inheritance received by the Aztecs from the Toltec past. As such Quetzalcóatl was held to be the inventor of civilization and its arts, not a mere introducer of them. And in both Toltec cosmogony and Aztec tradition he was invariably held to be native-born. The *Leyenda de los soles* treats him as a local creator deity, while the *Anales de Cuauhtitlán* register his birth in Tollan as a result of parthenogenesis.<sup>15</sup> There are many other traditions, but virtually all agree on his native origin. The historical Quetzalcóatl appears to have been a son, or possibly grandson, of the conqueror Mixcoatl. Later in life, as a priest-king of Tula, he lost a struggle for power and was forced out

<sup>13</sup> Gordon, *Before Columbus*, 170, 52–53.

<sup>14</sup> Wauchope, *Lost Tribes*, 6, *passim*. By including the Andean world Professor Gordon stretches Mesoamerica out of meaningful shape. Mesoamerica is commonly understood to be comprised of the area between Mexico's Pánuco and Lerma River basins in the north and central Honduras in the south, or that area which is delineated by common calendrical and agricultural adaptations.

<sup>15</sup> *Codice Chimalpopoca* (Mexico, 1945), 120–21, *passim*; 6–8. Also see *Florentine Codex*, ed. and tr. A. J. O. Anderson and C. E. Dibble, 1 (Santa Fé, 1950): ch. 5.

of the country, apparently in 987 A.D.<sup>16</sup> Maya sources, which provide exact dating, pick him up at this point. He and his partisans gathered an army and invaded the Maya world; after a violent conquest he established his capital on the older one at Chichen Itzá. There he imposed the feathered serpent cult, for which there is archeological evidence along with other Toltec cultural imprints. He thus became known in Maya tradition as Kukulkan, no culture hero from the east, as Professor Gordon imagines, but a brutal military conqueror from the west. The celebrated Mayan prophets, to whom we shall return, never identified Kukulkan or Quetzalcóatl with their prophecies, nor is there any known primary source that connects Kukulkan with the much earlier people of Itzá who, according to obscure legend, came from the northeast by sea. "Kukulkan," concludes J. Eric S. Thompson, "was but a flash in the Maya pan."<sup>17</sup> Gordon is equally uninformed when he claims that the Inca cherished the same tradition. Like the Tolteca, they conceived a local creator-deity, a culture hero of the Andean world who invented the civilized arts. He went nameless but had many titles, one of which, *Wiraqoca*, was hispanized into "Viracocha." He bore no relationship to bearded white men, benevolent strangers, or Plumed Serpent cult in fancy or in fact.<sup>18</sup> There is not and never was a generalized indigenous American Plumed Serpent myth. Nor is there any evidence that the Greek serpent-men were "Plumed Serpents" in the Mesoamerican sense, or vice versa. Considering the universality of the serpent as symbol in mankind's mythic creations, the comparison is narrowly and naively contrived.

As Schliemann's ghost is laid, two relevant points obtrude: Heinrich Schliemann, however much an amateur, knew his Homer. Professor Gordon did not know the primary Mesoamerican sources. The second point may follow from the first. The myth of Troy had an objective reality in Homer, but the Plumed Serpent myth, as propounded by Mr. Gordon, has none.

Another link is proposed in a series of parallels that the author perceives between Mediterranean traditions and the *Popol Vuh*, the sacred

<sup>16</sup> On Quetzalcóatl as both man and idea, see Miguel León Portilla, *La filosofía Náhuatl* (Mexico, 1959); Laurette Sejourne, *El universo de Quetzalcóatl* (Mexico, 1962); B. C. Hedrick, "Quetzalcóatl: European or Indigene?" in Riley, *Man across the Sea*, 255-65; Paul Kirchhoff, "Quetzalcóatl, Huemac, y el fin de Tula," *Cuadernos Americanos*, 84 (1955): 164-96; and Angel María Garibay Kintana, *Historia de la literatura Náhuatl* (Mexico, 1953). Quetzalcóatl is the great enigmatic figure of the Western Hemisphere. Since the sixteenth century interhemispherical diffusionists, largely following Spanish missionaries who had their own reasons for considering Quetzalcóatl non-American, have made him out to be a Jewish St. Thomas the Apostle, a Roman, an Icelandic missionary, a Carthaginian, an Irish saint, an East Indian, and in the present case, a misplaced Mediterranean. For details, see Manuel Orozco y Berra, *Historia antigua y de la conquista de México* (Mexico, 1960), 1, ch. 5, and Vicente Riva Palacio, *México a través de los siglos* (Mexico, 1940), 1, bk. 3, ch. 2.

<sup>17</sup> J. Eric S. Thompson, *The Rise and Fall of Maya Civilization* (Norman, 1966), 265.

<sup>18</sup> For details, see John H. Rowe, "Inca Culture at the Time of the Spanish Conquest," in *Handbook of South American Indians*, 2 (Washington, 1946): 293 n. 28.



book of the ancient Maya. Ostensibly these apparent cognates are held to indicate pre-Columbian contact. For the most part Professor Gordon merely elaborates what we already know, that mythologies the world over show striking similarities. Like others who have cited the *Popol Vuh* and similar materials, Gordon's lack of familiarity with it permitted a misapprehension that he was in direct contact with the ancient Indian mind and its ideas and could therefore draw straight comparative lines between Old and New World texts. What he did not understand is that ancient Indian traditions were oral long before they were written and that they lost something—perhaps a great deal—when they were subjected to the limitations of the Roman alphabet and the impositions of a foreign syntax. Even though the Indian(s) who first translated and edited the *Popol Vuh* remain anonymous, we know from critical analysis of other ancient Mesoamerican texts and commentaries on them that the only Indians capable of such labors were those who had been trained to it by Mendicant mentors. In the course of their education they were invariably Christianized and Latinized and could not escape the refractive power of such a lens. One must therefore question whether the ideas and traditions taken from antiquity were being reported as they actually existed or whether the moral values and intellectual discriminations of a foreign culture were operating as a mesh through which ancient ideas were being forced and thus inadvertently distorted. It is entirely possible, even probable, that Professor Gordon was correlating the work of an Indian editor who was himself closer intellectually and religiously to some of the European texts with which his work is being compared than he was to the ancient Indian traditions.<sup>19</sup> Comparatively speaking, the works of Mayan prophecy, like the books of Chilam Balam and Nahau Pech, provide suggestions of Christian tenets and moral distinctions having been read in. Quite aside from these hazards, the great Chilam Balam of Chumayel first made his celebrated prophecy of Spanish arrival and the new religion when the Spaniards were already swarming over the Caribbean.<sup>20</sup> They were using the Carib tongue, which the Maya knew, and when one considers the incredible amount of religious propaganda the Spaniards employed, the clairvoyant powers of the seer are diminished even as the erstwhile religious cognates he expressed are rendered suspect. These are not reliable sources upon which to predicate pre-Columbian contact.

Many of the friars who taught Latinity to the Indians themselves wrote of the Indian past, and their works comprise an invaluable body of sources. But it must be kept in mind that, with few exceptions, they

<sup>19</sup> There is yet a further question about the editing of the first Indian edition carried out by the cleric who discovered the manuscript. Some aspects of this problem are discussed more fully in R. C. Padden, *The Hummingbird and the Hawk: Conquest and Sovereignty in the Valley of Mexico, 1503-1541* (Columbus, 1967), ix-xvi.

<sup>20</sup> See Ralph L. Roys, *The Book of Chilam Balam of Chumayel* (Norman, 1967).

labored under the historical assumptions of *Genesis* and on that account postulated diffusion even if it required distortion of the evidence. Witness the otherwise trustworthy Fray Diego Durán in action: his source was an ancient Indian manuscript that told of tribal migration to Mexico from the north. Pictographs plainly showed a volcanic eruption with an accompanying earthquake; the air was filled with volcanic ash and a hapless Indian had fallen into a gaping seam as the earth opened beneath his feet. Durán interpreted the scene as an Indian version of the *Book of Numbers* wherein the earth swallows up Korah, Dathan, and Abiram. The ash was identified as the manna with which God sustained the Hebrews in the desert.<sup>21</sup> On the basis of such interpretation latter-day diffusionists could contend the Indians' pre-Columbian knowledge of the Bible.

Professor Gordon considers Mesoamerican sculpture to be a picture gallery that unmistakably shows the racial origins of its original subjects. Among many examples he includes a striking photograph of what is described as a Postclassical Mixtec head from Oaxaca: "The black color and the features, such as thick lips, leave no doubt in anyone's mind that the artist has portrayed a Negro."<sup>22</sup> The presence of a Negro in this time and place constitutes another link in Professor Gordon's chain. But at what cost? He has reduced analytical criticism, without which there can be neither artistic nor archeological determination, to a state of simplistic absurdity. Apparently, what you are looking at is nothing more than whatever it appears to be. On that basis, Preclassical figurines, with their typically too-large heads, puffy faces and mongoloid eyes, would necessarily represent a society of hydrocephalics. The diffusionist whom Gordon follows here has elsewhere described this technique as a science of "faciology."<sup>23</sup> An alternative explanation of the head, if it is Mixtec and Postclassical, suggests that the thick, downturned lips exemplify a widely used man-tiger stylistic convention based upon a legendary transformation of beast into man that was present in Mesoamerica from early Monte Albán times. The black color, if not inherent in the stone itself and therefore incidental, could represent what was later known as the black *ulli*, a staining pigment commonly applied to the bodies of priests and gods and having nothing at all to do with an attempt at a realistic portrayal of skin pigmentation.

It would seem profitless to discuss in detail the rest of Professor Gordon's links. He devotes an entire chapter to the evidence of language without producing any linguistic evidence. Competent linguists simply do not compare isolated words of unrelated languages in order to dem-

<sup>21</sup> Fray Diego Durán, *Historia de las indias de Nueva España y islas de Tierra Firme* (Mexico, 1951), bk. 1, ch. 1.

<sup>22</sup> Gordon, *Before Columbus*, 22.

<sup>23</sup> Alexander von Wuthenau, "Representations of White and Negro People in Precolumbian Art," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 109-10.

onstrate historical relationships. No one has yet established a continuity of linguistic families between the hemispheres in the pre-Columbian period. His chapter on ancient Greek authors is a rehash of previous speculations, contributing nothing of evidential value to his thesis. Materially, he offers a number of freak finds as proof of contact: an excavated Roman head, Hebrew and Roman coins, and inscribed stones and wall inscriptions. There has been a reluctance to accept such evidence. But even if it should be genuine it could prove nothing beyond a specific episode of pre-Columbian contact, and there is nothing earthshaking in that. Non-diffusionists are increasingly willing to concede that from time to time there could have been accidental landfalls and shipwrecks that resulted in contact. What is lacking is any apparent connection between these freak finds and Mesoamerican cultural development. One specialist in this area concludes that: "Items like these have so often proved elusive, unreliable, faked, or with such other disabilities as evidence that they must be ignored for practical purposes."<sup>24</sup>

One searches in vain for a coherent methodology in this work, for some systematic formulation of analytical criteria and evidence of its application. On a basis of superficial similarities Professor Gordon learnedly proposes "genetic relationships" between megalithic structures from Malta to Stonehenge, as borne out by "interlocking major features and details." "Genetic" is an unfortunate qualifier here; the interlocking major features and details are as follows: Malta and Stonehenge have horseshoe formations, and both are oriented toward celestial phenomena; a megalith at Stonehenge has carved on it a "distinctly Mycenaean" dagger; spiral figures carved on some of the monuments resemble the spiral form of the zodiac as represented on Egyptian remains; two such spirals carved on a Maltese temple are to be compared with two spirals on the Phaistos Disc from Crete, one other detail of which, in yet another context, is compared with a detail of an Aztec glyph and held as proof of pre-Columbian contact. Competent analysis requires that the possibility of convergence as a factor be examined. We know that the developmental processes of architectural traditions tend to produce structures of remarkable likeness in cultures widely separated in time and space. It is probable that structural analysis of these megaliths would demonstrate quite different derivations of these superficially similar monuments. But in lieu of analytical sufficiency it is "faciology" over and over again.<sup>25</sup>

Although Professor Gordon began with the declared intent to follow the primary sources to their logical conclusions, contrary opinion notwithstanding, he appears to have done precisely the opposite. Mesoamerican primary sources, both historical and archeological, argue persuasively that it was the American Indians themselves who created their brilliant

<sup>24</sup> Sorenson, "Significance of an Apparent Relationship," 223.

<sup>25</sup> Gordon, *Before Columbus*, 80, 93.

civilizations. Mr. Gordon rejects those sources and on a basis of diffusionist opinion proclaims the Mesoamericans to have been wretched dummies incapable of any such creation or development by themselves. From that point it is all downhill. The drawback is that his thesis, by its estrangement from Mesoamerican primary sources, is fatally devoid of historicity.

If *Before Columbus* is stereotypical of the many diffusionist arguments that have gone before it, *Man across the Sea* is not. Quite the contrary, it suggests the end of traditional transoceanic diffusionist postulation, especially as epitomized in the former work. As its title suggests, *Man across the Sea* is concerned with problems of pre-Columbian contact between the hemispheres and had its origin in a symposium on that theme held by the Society for American Archaeology in May 1968. Most of the papers presented are included in the present volume, together with others already published by the Southern Illinois University Museum in 1969. The volume is divided into three sections on theory, transoceanic contacts, and cultural geography respectively, with editorial commentaries.

Generally considered this book is not likely to be cheered by doctrinaire interhemispherical diffusionists. In spite of exhaustive research in ax and adz hafting, watercraft and sailing, ceramics, and intensive analysis of plant and animal evidence, there is still no proof, no hard evidence on which to predicate pre-Columbian contact and diffusion from the Old World to the New or vice versa. In fundamental theory the editors and commentators appear to agree that man is inventive after all. For some time now historians of culture change have demonstrated that it is invalid to assume the mutual exclusivity of diffusion and independent invention in human cultural development; they are differing aspects of a larger and more complex process of culture change, which neither can in itself explain. The abundant historical evidence of Spanish entry into the New World shows the adaptive process combining both evolutionary and diffusive mechanisms within the more general processes of transculturation.<sup>26</sup> The editors of *Man across the Sea* appear to agree with the historical argument in denying that diffusion and evolution as cultural processes are mutually exclusive, since the same integrative mechanisms seem to function in either case.

Methodological criticism is more pointed. David H. Kelley, writing on "Diffusion: Evidence and Process," warns that it is not possible to postulate diffusion credibly by mere compilation and comparison of artifacts or traits or by equally superficial comparisons of function. The in-

<sup>26</sup> See R. C. Padden, "Cultural Change and Military Resistance in Araucanian Chile, 1550-1730," *Southwestern Journal of Anthropology*, 13 (1957): 103-21; Padden, *The Hummingbird and the Hawk*; and Charles Gibson, *The Aztecs under Spanish Rule* (Stanford, 1964).

tegrative process can so change the diffused form that it is rendered unrecognizable; the trait is too much altered in the process and is therefore lost as evidence. "The criterion of functional equivalence between compared systems or artifacts as a test for historical relationships," he concludes, "seems almost completely invalid."<sup>27</sup>

Jon Muller takes a brilliantly critical look at "Style and Culture Contact," arguing that comparisons based on form alone—outward shapes and visible characteristics—are at best questionable. Such similarities, whether of massive structure or decorative detail, are often shown to be superficial by structural analysis, which entails thorough investigation of the cultural and ecological conditions surrounding the objects being compared. He seems to be saying that the cultural history of the objects being compared shows their chains of derivation, which, if different, nullify the basis of comparison. One significant upshot of this thesis is that diffusionists may no longer argue contact and diffusion on the ground that the compared forms are too complex to have arisen in two places independently without first having applied structural analysis to them. If one does not know how and why compared forms came into being, one cannot know that they are of the same derivation and hence comparable. These points, along with Mr. Muller's keen analysis of stylistic criticism, have profound implications for future stylistic comparisons.

Though the need for establishing genuine equivalence between compared traits is sharply drawn in this volume, the need for equivalence between "problem area" designation and the actual distribution of compared traits is not. There is no such penetrating examination of the diffusionist's arbitrary delimitation of problem areas within which he postulates diffusion from point A to point B. In spite of the papers on methodology and theory, otherwise informative and useful, the reader is left to wonder why diffusionists so often fail to consider the distribution of the traits they are comparing beyond their immediate "problem" areas. If, for example, the traits described as common to both Japan and Mexico and thus held to be indicative of diffusion are also to be found in Morocco and the Levant and wherever, by what definition is there a trans-pacific problem area? And does not the existence of the trait outside the problem area invalidate the premise of diffusion within it? These questions were raised by John H. Rowe several years ago, but in spite of responses by diffusionists they have not yet been satisfactorily answered.<sup>28</sup>

On the positive side *Man across the Sea* indicates that in recent years

<sup>27</sup> David H. Kelley, "Diffusion, Evidence and Process," in Riley, *Man across the Sea*, 61–62. Mr. Kelley points up the urgent need for more sophisticated analysis of the processes of culture change, a position with which historians readily agree. But at the same time one can but wonder why so many of those who postulate diffusion seem reluctant to take into account H. G. Barnett, *Innovation: The Basis of Cultural Change* (New York, 1953), and Margaret T. Hodgen, *Change and History* (New York, 1952).

<sup>28</sup> John H. Rowe, "Diffusionism and Archaeology," *American Antiquity*, 31 (1965–66): 334–37; Stephen C. Jett and George F. Carter, "A Comment on Rowe's 'Diffusionism and Archaeology,'" *ibid.* (1966): 867–70; Heyerdahl, "An Introduction to Discussion of Transoceanic Contacts."

there has been a shifting of gears and directions and that some scholars who continue to be intrigued by the idea of transoceanic diffusion have abandoned the taxonomical-comparative approach of the old scientism and are reaching for the new. As Carl O. Sauer cogently noted twenty years ago, the heart of the transoceanic contact and diffusion problem is that one is dealing in the present with a past that does not come again and cannot be verified experimentally.<sup>29</sup> That obstacle was more intimidating to his generation than to ours; however much scholars in disciplines outside the confines of pure science sought to emulate scientific method they could not achieve the certainty of predictability that was so essential to scientific method itself and to the validation of proof as scientific. There were inescapable consequences, one of which was a foreshortening of theoretical vision with an attending misapprehension of potentialities. Looking at the future from within the discipline of sociology a quarter century ago, for example, who could foresee its present dimensions? Since the splitting of the atom, however, science has seized upon probability as avidly as it had predictability in earlier times, and, while predictability continues to be a useful element of scientific method, calculated probability has arisen as an empirical alternative to certainty. It is this that has freed the social sciences and humanities from their former bondage; and in the long run it may well prove to have been this, rather than the unleashing of atomic fire, that labels the event Promethean.

George F. Carter, summing up his researches on the question of "Pre-Columbian Chickens in America," provides *Man across the Sea* with a fair example of how the new scientism is changing the search for evidence and perhaps the nature of the diffusion problem itself. The chicken debate has been going on since the sixteenth century. Spaniards could and could not remember—but could never agree—on whether or not the chicken was already present when they overran the Indies.<sup>30</sup> Professor Carter approaches the problem in multiple ways:<sup>31</sup> he delves deeply into zoological and genetic data; he exhausts the linguistic aspect; he compares the sociocultural complexes that surround the bird wherever it is found, especially in America, Polynesia, and Asia. His accumulated evidence establishes a possibility of pre-Columbian introduction. Next he painstakingly constructs a historical model of the origin and dispersals of the chicken in Europe, fixing precise rates of diffusion. Then

<sup>29</sup> Carl O. Sauer, *Agricultural Origins and Dispersals* (New York, 1952), 1. This brilliant contribution is once again available under the title *Seeds, Spades, Hearths, and Herds* (2d ed.; Cambridge, Mass., 1972).

<sup>30</sup> The basic literature is E. Nordenskiöld, "Deductions Suggested by the Geographical Distribution of Some Post-Columbian Words Used by the Indians of South America," in *Comparative Ethnographical Studies*, no. 5 (Göteborg, 1922), 1–46; Ricardo E. Latcham, *Los animales domésticos de la América pre-columbiana* (Santiago, Chile, 1922); Sauer, *Agricultural Origins and Dispersals*; R. M. Gilmore, "Fauna and Ethnology of South America," *Handbook of South American Indians*, 6 (Washington, 1959): 345–464.

<sup>31</sup> At the risk of oversimplifying his sophisticated methodologies I have taken Mr. Carter's procedures out of order and context in an attempt to conserve space.



he builds a duplicate American model—and finds a diffusion rate that is by comparison so much faster that European introduction of the chicken is rendered incredible. He further summarizes the argument for Spanish and Portuguese introduction of both Asiatic and European chickens after 1492, but the case for pre-Columbian Asiatic origin is more convincing by far. The archeological record, one chicken bone, should settle this question absolutely.

Perhaps the clearest example of calculated probability one can suggest in the context of this paper is Paul Tolstoy's "Culture Parallels between Southeast Asia and Mesoamerica in the Manufacture of Bark Cloth." He theorizes that the highly technical process of bark-cloth making diffused from Southeast Asia to Mesoamerica in the pre-Columbian era. It is his method that is most relevant here.<sup>32</sup> Briefly, Professor Tolstoy identifies and graphs out the obligatory and optional decisions—crossroads at which the bark-cloth maker is faced with a functional or material choice in the course of manufacture. He likewise lays out the actual options that are available to the worker in taking these decisions. High correlations of decisions made in the presence of similar alternatives are held to increase the probability of imitation and to decrease the likelihood of independent invention. His correlations appear to be high enough to pose the ultimate question: how much calculation is enough? On the face of it probability would appear to be adequately calculated when contact and diffusion, even without apparent means, is easier to accept than duplication of the statistics of bark-cloth manufacture merely by chance. Leaving aside the crucial matter of historicity, to which we shall return, there is a reservation to be considered in this context. The value of calculated probability is necessarily modified by the presence or absence of corroborative historical sources; mere probability can be illusory. The given statistics and their correlations on Asia and Mesoamerica, for example, have apparently not been subjected to analytical comparison with those of bark-cloth manufacture over the rest of the world. Until that is accomplished it is difficult to see how the present correlations in themselves can be held indicative of any specific, directional process of diffusion or how the existence of a transpacific diffusion "area" can be assumed.

These approaches to the question of interhemispherical diffusion coincide with the much earlier interests of cultural geographers in the problems of plant migrations.<sup>33</sup> Phytogeographers have long suspected interhemispherical contact. If a cultigen could not possibly have spread

<sup>32</sup> Paul Tolstoy, "Culture Parallels between Southeast Asia and Mesoamerica in the Manufacture of Bark Cloth," in *Transactions of the New York Academy of Sciences*, 2d ser. (1963) 25: 646–62; Tolstoy, "Method in Long Range Comparison," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 69–89.

<sup>33</sup> See, for example, Jacques Barrau, ed., *Plants and the Migrations of Pacific Peoples* (Honolulu, 1963); J. W. Purseglove, *Tropical Crops* (New York, 1968); Sauer, *Agricultural Origins and Dispersals*; and Herbert G. Baker and G. L. Stebbins, eds., *The Genetics of Colonizing Species* (New York, 1965).

by natural means, then human intervention seems the only alternative explanation. The final section of *Man across the Sea* thus includes updated studies of the coconut, the bottle gourd, and the sweet potato and of squash, maize, cottons, and beans. As proofs of diffusion they are either negative or inconclusive, and they generally fail to support the fancies of traditional diffusionists, yet these studies are not without provocative suggestions. The work of Herbert G. Baker, this section's commentator, argues persuasively for the probability of human intervention in the dispersal of the kapok tree in pre-Columbian times.<sup>34</sup> It is worth noting the spread of disciplines represented in this section: two geographers, two geneticists, one biologist, one ethnobotanist, one anthropologist, and three botanists. One finds here methodological and speculative levels more consistently impressive than elsewhere in the volume.

The telling difference between the traditional advocates and the more recent proponents of diffusionism is that the former tended to speculate on the probable validity of gratuitous assumptions,<sup>35</sup> while the latter attempt to calculate probability on a basis of data analysis. The old route of easy abstractions invariably led to gross oversimplification, implying commensurate irrelevance for historical sources. Probability, if adequately calculated, itself becomes a kind of ahistorical evidence, neither documentary nor circumstantial; yet it points up directions in which historical evidence might be researched and developed. But probability never tells us how or who and seldom indicates when. Whatever the disciplines involved and the methodologies employed in the calculation of probability, the problem to which it must be applied, whether that problem be the origins of agriculture or cultural diffusion, is preeminently historical in nature.<sup>36</sup> But historical problems are not often amenable to biological or anthropological solutions. And there is the rub. Most of the methods by which probability is calculated tend to be essentially synchronic. According to hematologists, for example, the blood group genetics of Polynesia suggest the conclusion that modern Polynesians result from a gene pool derived from Indonesia, western Polynesia, and South America.<sup>37</sup> This data possibly has relevance for

<sup>34</sup> Herbert G. Baker, "The Evolution of the Cultivated Kapok Tree: A Probable West African Product," in D. Brokensha, ed., *Ecology and Development in Tropical Africa* (Berkeley, 1965).

<sup>35</sup> An example is provided by Gordon F. Ekholm, "Transpacific Contacts," in Jesse D. Jennings and Edward Norbeck, eds., *Prehistoric Man in the New World* (Chicago, 1964), 498-501.

<sup>36</sup> See Ping-Ti Ho, "The Loess and the Origin of Chinese Agriculture," *AHR*, 75 (1969-70): 1-36. It is not my intention to obscure the meaningful distinctions between historical problems and nonhistorical problems in history. My point is that even though multidisciplinary calculations have brought the question of pre-Columbian chickens down to the matter of one tiny bone, if and when that bone is found the historical question will be primary.

<sup>37</sup> Ruben Lisker, "El origen de los grupos humanos en América: Serología y hematología en general de los amerindios y sus posibles relaciones trans-pacíficas," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 43-51; Roy T. Simmons, "The Blood Group Genetics of Easter Islanders (Pascuense), and other Polynesians," in Thor Heyerdahl and Edwin

postulating migration of the sweet potato out from South America; but, for the historicity upon which any such postulate must eventually depend, it is signally irrelevant. As we have seen, anthropologists commonly amass culture traits for purposes of comparison and, on occasion, predication of diffusion. Although the traits have been taken from widely separated time levels and sources, for analytical and comparative purposes they are assumed to be constant. But in separating the traits from their chronological and spatial identities the entire analytical structure is divested of historical reality and validity. Any probability value that may have been calculated is thereby compromised with respect to the predication of actual historical process. Inescapably, any reconstruction of the past will lose its historicity in direct proportion to the departure of its elements from chronological and spatial sequence.

This is where diffusionists have so often come to grief: they seldom find it possible to accommodate theory to historicity, which, in historical questions, has always been the ultimate test. Some simply refuse, arguing that unsolved problems of nonmatching sequences and distribution—the essence of historicity—are irrelevant before the implications and demands of anthropological theory. Strangely enough, one person who makes that argument pleads in the next breath that “solving the mystery of man’s past” is her central goal.<sup>38</sup> Historians have not rejected diffusionist claims out of obtusity or fear of new ideas. They have refused to accept the postulation of historical processes and events that are patently lacking in historicity.

Probability, when precisely calculated from sources and data commensurate with historical methodologies, is an invaluable tool for reconstructing the past, especially a past filled with shadows. For those of us who work in ethnohistory calculated probability is indispensable. It seems evident that whatever their disciplinary foci, exponents of diffusionism must eventually come to terms with historicity because it is the *sine qua non* for comprehending the development of human culture and for any credible demonstration of the phenomena accompanying that development. The problems are truly formidable and will not, as in the past, be obviated by simplifying their dimensions to fit inadequate methodologies or to satisfy theoretical limitations.

The editors of *Man across the Sea* believe that the question of what constitutes acceptable evidence is the major problem facing scholars today in any discussion of diffusionism. And one could agree that for diffusionists the problem of evidence is as old as the idea itself. But the diffusion of culture is a historical question ultimately and in the realm of

N. Fejndon, Jr., eds., *Reports of the Norwegian Archaeological Expedition to Easter Island and the East Pacific*, 2 (Stockholm, 1965), 333–43.

<sup>38</sup> Betty J. Meggers, “North and South American Cultural Connections and Convergences,” in Jennings and Norbeck, *Prehistoric Man in the New World*, 511–26.

historiography wherein it is to be resolved there is no confusion or misunderstanding as to the nature of evidence, nor is there doubt concerning the sufficiency of evidence. Moreover, in the society at large it is only through the most stringent preservation of its historicity that historical reconstruction can function as a trusted intellectual resource and as a viable mode of inquiry into human behavior. If you lose that, you have lost everything.

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## Reviews of Books

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### GENERAL

JACOB VINER. *The Role of Providence in the Social Order: An Essay in Intellectual History*. (Memoirs of the American Philosophical Society, volume 90. Jayne Lectures for 1966.) Philadelphia: the Society. 1972. Pp. viii, 113. \$2.00.

No informed reader can hope to derive unalloyed pleasure from these four brief, elegant lectures delivered at Philadelphia in 1966, given as they were just four years before the author's death. Nor, of course, does the editor regard this volume, lacking in footnotes and index, as a fitting memorial to an outstanding scholar—one of the greatest economists America has produced. Nevertheless, those who have been awaiting Viner's long-promised study of the European background to religious thought and economic society in America, which was announced more than a decade ago, must derive what crumbs of comfort they can. Moreover we may fervently hope that this is one of several posthumous works, a possibility suggested by Professor Fritz Machlup's helpful article, "What was Left on Viner's Desk" (*Journal of Political Economy*, 80 [1972]: 353–64).

Viner's lectures range widely in time, from the Greeks and Romans through the Bible to the nineteenth century, with special emphasis on the seventeenth and eighteenth centuries. The first, "The Cosmic Order in the Service of Man," presents the historical background to the main theme, chiefly from Christian sources, and concludes that optimistic providentialism had its roots in the Enlightenment and the secularization of religious thought, rather than in traditional Christian orthodoxy. In discussing "The Providential Elements in the Com-

merce of Nations" Viner concentrated on the concept of a providential relative abundance of necessities as compared with luxuries, and on the role of trade as a means of promoting the universal brotherhood of man. Lecture three, "The Invisible Hand and Economic Man," deals with secularized ethics and its applications to socio-economic issues, while the final address, on "The Providential Origin of Social Inequality," considers both the defense of providence in creating inequality and the justification of inequality in providential terms.

Although in his opening and closing remarks Viner adopted an austere view of the role and responsibilities of the historian of ideas, his lectures are characteristically urbane, witty, and erudite. While deliberately eschewing technical economic analysis he nevertheless offered illuminating comments on the intellectual background of modern economics. One can, of course, cavil at some of his opaque, if carefully worded, generalizations—for example, his insistence on the unity of "expressed opinion" in British social thought from 1660 to 1776 "with respect to general social policy bearing on class stratification, the rights and duties of the poor, the proper location of political power, the functions and limitations of public alms and private charity" (p. 96). But such reservations are far outweighed by a deep sense of gratitude for this reminder of a splendid scholarly life. And is it not fitting to conclude by urging his Princeton colleagues to mine more treasures from the rich store that Viner bequeathed to us?

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*Household and Family in Past Time: Comparative Studies in the Size and Structure of the Domestic Group over the Last Three Centuries in England, France, Serbia, Japan and Colonial North America, with Further Materials from Western Europe.* Edited, with an analytic introduction on the history of the family, by PETER LASLETT, with the assistance of RICHARD WALL. (A publication of the Cambridge Group for the History of Population and Social Structure.) New York: Cambridge University Press. 1972. Pp. xii, 623. \$37.50.

This important volume will doubtless be discussed for years to come. Earlier studies from the Cambridge Group for the History of Population and Social Structure have already lifted our understanding of the demographic history of England to a new plane. Now Peter Laslett and nearly two dozen other scholars from universities around the world have gathered together masses of new evidence on the nature of the domestic group in England over the past three centuries and have complemented it with comparable material from France, Japan, Serbia, Italy, Belgium, Holland, and North America. The bulk of the book is given to the presentation of this data case by case, with chapters on "Household and Family in Tuscany in 1427," "Size and Structure of Households in a French Village Between 1836 and 1861," "Rhode Island Family Structure: 1875 and 1960," and the like. But there is as well a heroic effort at integration and systematic cross-cultural comparison in the ninety-page introductory essay by Peter Laslett.

I cannot pretend to have fully digested this formidable enterprise. I am not an expert in the techniques of historical demography, have no firsthand acquaintance with the sources utilized by the authors, and lack even a thorough grasp of the abundant literature on the subject. But there may be some value in approaching this volume from the vantage point of a social historian who is more a consumer than a producer of demographic history. What can such an innocent learn from this book about the household and family in past time?

I found myself alternately excited, bored, and puzzled. The boredom came in a few papers that offered statistical data without general informing ideas—useful perhaps to specialists in the field but deadly to the outsider. More than offsetting these in number are some

excellent essays that utilize microscopic data to suggest interesting interconnections between family and society. With characteristic grace and lucidity, John Demos draws out the psychological implications of demographic materials from colonial New England. The anthropologist Robert J. Smith presents fascinating findings on the residential instability of households in eighteenth- and nineteenth-century Japanese cities and links them to characteristics of the local political system. Another anthropologist, E. A. Hammel, provides a penetrating look at the *zadruga*, the classic extended family of the Balkans, warning that the institution was "not a thing but a process" and that it displayed impressive continuity over time despite wide fluctuations in the simple statistical indicators commonly used to measure its vitality. Several papers—those by Klapisch on fifteenth-century Tuscany, van der Woude on seventeenth- and eighteenth-century Holland, Greven on eighteenth-century America, and Hayami and Uchida on Tokugawa Japan—point up interesting variations in family patterns related to social class, urbanization, modes of agriculture, and other factors. It is clear from contributions like these how much the social historian can learn from a closer look at the family.

The backbone of the book, however, is Peter Laslett's introduction, "The History of the Family." This major essay is bold, challenging, and elegantly written. It sharply assesses the current state of the field and offers shrewd comments about such issues as the relationship between societal belief systems concerning the family and actual behavior patterns. But its central core left me dissatisfied and somewhat baffled.

Laslett presents a classification system that can be employed to provide strictly comparable measures of household size and structure over time and space and tries to demonstrate the value of that system in a comparative analysis of five areas—England, France, Serbia, Japan, and colonial America. Two large questions are posed. First, how large was the typical household, defined strictly as people living together under one roof as a "coresident domestic group," across a broad temporal and cultural spectrum? Were families characteristically much larger in preindustrial societies of the



past, and did they shrink in the course of modernization? Second, how was the household structured in these various settings? Were complex, extended, or multiple family groupings once the norm? What circumstances created the simple nuclear family pattern dominant in the advanced societies of our own day?

For England since the late sixteenth century, Laslett's answers are both convincing and startling (though anticipated, of course, in earlier publications from the Cambridge Group). On the basis of data from a sample of one hundred English communities between 1564 and 1821 and subsequent census figures, he demonstrates that the mean household size was relatively small (less than five persons) and astonishingly constant until the very end of the nineteenth century. After that the average size of the English family decreased substantially, but the change came so late that the usual explanations—"urbanization" or "industrialization"—are obviously inadequate. Likewise, the simple nuclear family was not the product of modernization. It was practically universal in Elizabethan England, as much so as in the reign of Elizabeth II more than three centuries later. The only deviation from this pattern, and a quite unexpected one, was during the early phases of the Industrial Revolution when there was an abrupt but temporary rise in the proportion of complex households in both urban and rural areas. These results are thoroughly persuasive, and they demolish much conventional wisdom concerning the corrosive, atomizing, individualizing thrust of historical development.

It is Laslett's effort to set these findings into a broader comparative context that proves troubling. The key cases considered here are Bristol, Rhode Island, in 1689, a Japanese fishing village in 1713, Belgrade in 1733, and a northern French village in 1778, with sporadic references to roughly comparable data from other communities. The chief conclusions drawn from this exercise are that, with respect to family size, the English, Japanese, and French patterns look much the same, while Serbian and American families were significantly larger. As to structure, simple nuclear forms were nearly universal in England, France, and America; more complex families

were the rule in Japan and were a large minority (about a third) of Serbian households.

Just what are we to make of this? What most surprises Laslett, apparently, is that the differences between these societies were not more pronounced. Only Tokugawa Japan stands out as a culture dominated by a complex family tradition, and since it was selected for study precisely because it was expected to exhibit that characteristic, Laslett is impressed that even there households were not unusually large and that not many fewer than half of them were nuclear in form. This brings him to advance "the null hypothesis in the history of the family, which is that the present state of the evidence forces us to assume that its organization was always and invariably nuclear unless the contrary can be proven." It follows from this that "the form of the family" is "not capable of doing all the work which social scientists have seemed to expect it to do." Patterns of family organization will not serve us well as explanatory variables, whether dependent ("industrialization brought the nuclear family") or independent ("the dominance of the nuclear pattern in early-modern England predisposed the society towards industrialization"). A variable, after all, must *vary*. The major attempt at synthesis in a volume aimed at "opening up a new field of enquiry" thus seems to close off more unexplored territory than it opens up. If Laslett is indeed right, further comparative study of the dimensions of family life his classification scheme is designed to measure will be an utter waste of time.

Is he right? Laslett is diffident and tentative, as well he should be. The spectrum of cases analyzed is rather narrow and of limited representativeness. There is a serious methodological question, as the author admits, about the validity of generalizations based upon population listings at a single date. As Hammel notes, families are not things but processes; complex families might embrace only a fraction of a population at any one point and yet be experienced by a majority at certain crucial phases of the life cycle. (For a splendid case study illustrating this point, see Lutz K. Berkner, "The Stem Family and the Developmental Cycle of the Peasant Household: An Eighteenth-Century Austrian Example," *AHR*, 77 [1972]: 398-418.) One wonders, as in all such exercises in

comparison, how much of a difference is enough to make a difference. I myself was struck by the dominance of complex family forms in Japan and by their great importance in Serbia. Both areas seem fundamentally unlike England, France, and North America on this count, and it is not beyond the realm of possibility that this has some bearing upon other distinguishing features of those societies.

On the whole, though, the evidence seems sufficient to raise serious doubt that the simplest quantitative indicators of family patterns in past time—size and degree of complexity—are in themselves potent variables when isolated from other features of the family system. Perhaps this is a very useful thing to know, a valuable reminder that further efforts at comparative analysis of family life will need to be more broadly gauged, more refined, and more subtle. My disappointment with Laslett's synthesis, which will surely be the most widely-read piece in the book, is that it does not take the next step, does not forcefully point out the directions in which progress may be made. Laslett has just returned from an unsettling voyage of discovery, and what is most on his mind is that there is no Northwest Passage. Possibly he is right, but many of the essays in this volume suggest that there are untapped riches beyond the null hypothesis. A preliminary map of the shape of that territory is what we need now.

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*Third International Conference of Economic History, Munich, 1965. Volume 4, Demography and Economy: Papers and Report of Discussion*, edited by D. E. C. EVERSLEY, assisted by JANE S. WILLIAMS. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Congrès et colloques, 10.) Paris: Mouton & Co. 1972. Pp. 268. 48 fr.

The appearance of this collection of papers on demography and economy presented at a conference seven years ago is unlikely to arouse much interest in the field of historical demography, which, like Malthus's unchecked population, has been growing in geometric progression since then. (See the review article by Franklin F. Mendels, "Recent Research in European Historical Demography," *AHR*, 75 [1970]: 1065-73.) The preface to this volume

relates the unhappy publishing history of the venture, and the results are terribly uneven, ranging from arguments without data to data without arguments, with a few substantial articles scattered in between. Fortunately the excellent introduction by Eversley puts the papers into perspective and skillfully extracts the major themes: regional growth patterns, rural industrialization, origins of the labor supply, marriage patterns, fertility, mortality, and migration.

The four papers on Great Britain all cover familiar ground. Wrigley discusses the problems of family reconstitution from the registers of the now-famous parish of Colyton. Drake again makes the important point that marriage ages differed in Ireland and Norway not only according to social class, but also by sex within each class. Farmers married late, but chose young women; cotters married earlier, but chose older wives. Armstrong's paper does little more than review the extensive literature on the relationship between industrialization and population growth in England, but it may be profitably read as a background to Pentland's important article on "Population and Labour Growth in Britain in the Eighteenth Century." Pentland constructs indices of natural increase for seven series of English parish registers and finds that there appear to be roughly twenty-year cycles of population growth throughout the eighteenth century. He argues that these cycles are related to the low proportion of young adults in the population and their response to favorable economic conditions.

There are also four papers on Scandinavia, but Lassen's series of tables illustrating regional population change in Denmark from 1650 to 1960 and Friberg's statistics on population change in a Swedish mining district from 1650 to 1750 consist of little more than presentations of undigested data. Much more useful is Jutikkala's synthesis of a number of recent unpublished studies of migration in Finland during the nineteenth and twentieth centuries. He shows that there was more geographic mobility before the period of modern industrialization in Finland, because of the continual migration of agricultural laborers. One article in the collection that will be of general interest to social historians is Hansen's long summary of his book on the Danish aristocracy from 1470

to 1720. He describes not only the demographic characteristics but also the social composition of this group and makes the interesting case that the decline of the old aristocracy in the seventeenth century can be explained by an occupational shift into military careers, which led to higher mortality and also increased hindrances to marriage.

The rest of the papers are extremely short summaries of previously published or ongoing research dealing with the demographic history of particular nations or regions of Europe. Blaschke (on Saxony) and Deprez (on Flanders) briefly present the results of their well-known work on population growth in two regions of highly developed rural industry. The correlation they find between high regional population density and rural industry in the early modern period is also reported in an interesting piece on Bohemia by Petráňová. Kovacsics discusses the problem of estimating population change in eighteenth-century Hungary from the tax lists and censuses. The group of historians associated with Slicher van Bath summarize the state of their research into the social, demographic, and economic history of various regions of the Netherlands before 1800. Finally, there are brief reports of the four discussion sessions, which add a few scattered ideas and facts. None of the problems discussed in this volume have been solved in the seven years since the papers were first presented, but such an enormous amount of information has been added that it would be unwise to depend on this collection for more than an interesting introduction to some of the questions and for English and French summaries of some research in unfamiliar languages.

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HEINRICH OTTO MEISNER. *Archivalienkunde vom 16. Jahrhundert bis 1918*. Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 365. DM 28.

HELMUT MATHY. *Die Geschichte des Mainzer Erzkanzlerarchivs 1782-1815: Bestände—Organisation—Verlagerung*. (Recht und Geschichte, number 5.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. ix, 158. DM 24.

The *Archivalienkunde* by H. O. Meisner is a revised and expanded version of the author's

well-known *Urkunden-und Aktenlehre* (Leipzig, 1950). It is a systematic analysis of the various types of archival materials of the period 1500 to 1918, covering public and private, manuscript, typescript and printed, as well as visual and recorded materials. It is not, however, a general manual on archival science, as it is based overwhelmingly on German, especially Prussian, archives. This is not surprising in view of Meisner's long and distinguished career in German archives administration and training, most recently in the Zentralarchiv in Potsdam. One could characterize this work in general by saying that Meisner has done for German archives what Sir Hilary Jenkinson did for the English with his classic treatise (*A Manual of Archive Administration* [3d ed.; London, 1965]).

The book is divided into two main parts. The first, the more general one, consists of a theoretical and historical discussion of the main categories of archival records. The differences between "Urkunden," "Akten," and "Briefe" are meticulously set forth to a degree of refinement perhaps not always comprehensible to the practitioner in non-German lands. Considerable attention is paid to the history and technique of registry of archival materials and the important role of archives in modern documentation.

The second, more extensive part of the work is devoted to modern diplomatics—more specifically, to the analysis and identification of public documents by the application of three main methods: the systematic, the analytic, and the genetic. The systematic method involves chiefly the utilization of certain style characteristics (for example, use of personal pronouns, rank of sender and receiver) to determine the status of a document. The analytical method consists of an almost microscopic dissection of physical characteristics (such as paper, ink, watermarks, seals) and of internal ones (salutation, titles, dating system, main text, closing formula, and signature). Finally, the genetic method views the document from the point of view of its origin and evolution to its final form. This part of the work, describing the originating process of state documents and the specific administrative agencies and officials involved (again chiefly in Prussia), is no doubt of special interest and help to scholars who wish to work in

German archives (also, I hope, in those located in the German Democratic Republic). The final section consists of a glossary of technical terminology, a field in which Meisner has done pioneering work. There are numerous explanatory notes and a full bibliography.

The little book edited by Helmut Mathy is of particular interest to German archivists and to historians of archival science in general. It consists of a selection of fourteen documents, with a brief introduction by the editor, that shed light on the organizational development, as well as the vicissitudinous career of the Reichsarchiv of the elector of Mainz during the stormy years of the French Revolution. The special importance of this archive derives, of course, from the fact that the archbishop and elector of Mainz held the office of arch-chancellor of the Holy Roman Empire, and his archives were thus the repository of documents that constitute an important source for the whole political history of the Reich (they were incorporated in the Vienna Haus-Hof-und Staatsarchiv during the nineteenth century). The documents presented here contribute within a limited context to our fragmentary knowledge of the Mainz archives, whose full history remains to be written and whose unique contents have been hitherto rather neglected by historians. It contained, for example, original copies of the Golden Bull of 1356, the Augsburg Confession, and the Treaty of Westphalia.

The central document in this book is a lengthy, detailed proposal by the archivist, Johann Baptist Kissel, written in 1781, for the establishment and administration of a separate archives (Reichsarchiv) for these imperial documents, based more or less on the modern provenance principle. The most frequent user, by the way, of the Mainz archives in those days was the famous Swiss historian, Johannes von Müller, to whose home—blissful bygone days!—the desired original documents were dispatched by the obliging archivist.

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ELLIS RIVKIN. *The Shaping of Jewish History: A Radical New Interpretation*. New York: Charles Scribner's Sons, 1971. Pp. xxxi, 256. \$7.95.

Professor Ellis Rivkin of Hebrew Union College here offers a "radical new interpretation" intended to "redesign Jewish—and as a consequence world—history." He claims that Jewish history is unique "in a phenomenal sense." His "operative principle" is "utterly simple: The problems of Jewish history can be understood by means of the unity concept. For most of Jewish history, this concept was the affirmation that God was one and omnipotent. . . . Each successive form of Jewish history represents a solution to problems posed to the idea of unity by changing historical circumstances." Rivkin spells out his interpretation in reference to the standard themes of an Occidental and European conception of the histories of Jews: the Biblical period, Pharisaism and Talmudic Judaism, the medieval West, the rise of capitalism, the emancipation, nationalism, "The Road to Auschwitz: The Disintegration of Nation-State Capitalism," and "The Road from Auschwitz: The Emergence of Global Capitalism." The Jews scarcely appear in these last chapters, which focus upon the cold war.

A book with such a pronounced theological orientation is bound to raise serious problems for the ordinary historian. But the primary one is, does the author exhibit awareness of the intellectual antecedents of his theology? In calling his interpretation "radical" and "new," Rivkin makes one wonder whether he has read Deuteronomy, which posits the same "principle," or any of the subsequent sacred literature of Israel and of Judaism. At the beginning of the modern period in Jewish historiography, a century and a half ago, Immanuel Wolf wrote, "What is this idea that has existed throughout so much of world history? It is the idea of unlimited unity in all. It is contained in the one word YHWH which signifies indeed the living unity of all being in eternity." Wolf fails only to tell us this is "radical" or "new." Rivkin posits such a continuum as "Jewish history" without asking what justifies the treatment as a single continuous entity of the disparate and discrete groups known in various settings as Jews, especially since the traits thought in one place quintessentially Jewish in another in no way were associated with Jews. True, Jewish theology creates the category of "Jewish people," which functions like "church" for Christian theology—and is every bit as abstract and

remote from the data of the churches here and now. But, entirely lacking in theological sophistication and intellectual self-awareness, Rivkin is unable to see the force of these questions.

If he is not much of a theologian, Rivkin fails also to establish a claim to be taken seriously as a historian. This is for simple reasons. First, he passes off as facts a great many questionable or moot propositions. Side-stepping the central issues of historical knowledge, he pays no heed either to problems inhering in sources or, indeed, to whether any sources at all say what he says is so. Second, he resolutely ignores the whole of scholarly literature on each and every point; the book lacks not only footnotes but also a bibliography. The entire apparatus of scholarship is missing. Third, the book is full of fake arguments, for example, "No one really knows, simply because there are no records that tell us how this composite became the Pentateuch. . . . I therefore see no cogent argument against approaching the Pentateuch from a very different angle of vision." I last heard that in junior high school. Fourth, the definition of "Jewish history" omits virtually the whole of the non-European segment, as if it simply never lived. Fifth, Rivkin's grasp of the various histories necessarily included in his account is exceedingly fuzzy, as is understandable. To take one minor instance, he regards the "intellectual world of the Sassanians" as "nonrational and disorderly," and this explicitly is applied to disorder in economic, social, and political terms. But everyone knows the hierarchical and highly orderly theory of Sassanian politics and government; the consequent explanation of the "disorder" of the Babylonian Talmud—which some think very logically put together indeed—is based upon incorrect suppositions. And so it goes. Finally, for a historian the book's florid and verbose style is painful; but I should not condemn theologians to sit through it either. Nearly every page will yield glories such as this: "The downtrodden trade off trust for the psychic reassurance of ideas saturated with fantasies of ultimate liberation. So it was with Christianity, again and again. So it was with Judaism, again and again. So it has been with Marxism, again and again." And so on.

This "radical new interpretation" may have

the redeeming social value of inspiring Reform Jewish Sunday school students, but its pretentiousness, ignorance, and vulgarity of thought and expression hardly commend it for the careful consideration of a less partisan, noncaptive audience.

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PHILIP P. ARGENTI. *The Religious Minorities of Chios: Jews and Roman Catholics*. New York: Cambridge University Press. 1970. Pp. 581. \$22.00.

Exposed to invasions and occupations, it was natural for Chios to acquire minorities. One of them, a small one, was the Jews. They probably settled there first during the great dispersion of the Hellenistic period, but they began to live as a community in the eleventh century, under the Byzantines. The author has related, all through his study, the fate of the Jews of the island to that of the Jewry at large, apparently because of the scanty information at his disposal. It was the Emperor Constantine IX Monomachus who assigned the Jewish community of Chios to the Orthodox monastery, *Nea Moni*, with the obligation that they pay a poll tax, called *kephaletion*. An interesting discussion ensues, expressing various scholarly points of view, on the connection between the Jewish *kephaletion* of the island and that of the Byzantine Empire as a whole. The Byzantine period is the most extensive and the best documented in the book, though in parts it reads like a legal treatise. The lot of the Chios Jews did not improve under Catholic rule, for the Catholic Church pursued a policy of persecution. There were instances, however, when the secular power was not faithful to it. During the Genoese rule of the island (1346–1566), thanks to the support of the *Mahona* (Genoese secular power), the Jewish community prospered, particularly as middlemen for the mastic trade. Under the Ottomans, the Jews fell in the category of *Dimmis* (tolerated infidels) and they were treated like the other non-Moslems. They had to pay *cizya*, a capitation tax. While there has been a confusion between *cizya* and *harac* (land tax)—the latter having changed meanings in the course of time—Argenti is not right in maintaining that "by the Ottoman



period the poll-tax was designated as *kharadj*" (p. 149). The term *cizya* was still used when *harac* was substituting for it. Even Gibb and Bowen, whom the author cites, state that the improper usage of *harac* for *cizya* seems to have been adopted earlier than the nineteenth century, "though not in either annals or official documents" (*Islamic Society and the West*, 1 [1957]: 252). Ottoman rule was not harsh on the Jews and they favored it. It was their pro-Ottoman attitude during the Greek revolt of 1821 that accounted for the wave of anti-Semitism that spread among the Greeks.

The Roman Catholics were the largest minority in Chios. Under the Western powers, they were naturally the most dominant. When the Ottomans occupied the island, on the other hand, they were at a disadvantage, as the pope was outside the sultan's territories, and any contact with the Vatican might be interpreted as political disloyalty. During the period 1566-1695 relations between them and the Orthodox, who were all Greek and constituted the great majority of the population, were surprisingly friendly. There were families composed of Orthodox and Catholic members; in the "mixed churches" there were altars for the two rites; the council of the Chios community was constituted of Orthodox and Catholic *demogerontes* (elders). But these peaceful relations were disturbed at times—the Orthodox resented the proselytizing efforts of the Catholic Church. More frequent were the quarrels among the Catholic religious orders of the island. In 1694, at the time of the war of the Holy League against the Ottomans, Venice occupied Chios. The Catholic population manifested its sympathies and assisted her. In 1695 the Venetians were compelled to leave the island. With them a substantial number of the Catholics departed and settled in Venetian Morea and the Ionian Islands. This reminds one of the "great migration" of the Serbs in 1690, when they followed the retreating Austrian armies, whom they had helped during the same war against the Ottomans, and settled in southern Hungary. After 1695 the number of Catholics of Chios began to dwindle until in the twentieth century it became negligible.

As usual, Argenti's book is well documented. There is even a long section (pp. 373-537) containing whole documents. He is also objective.

But the treatment of the subject is uneven and there is not much coherence—it is fragmented. Yet the study throws considerable light on the two religious minorities of his native island and the broader aspects of Jewry and Catholicism.

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O. EDMUND CLUBB. *China & Russia: The "Great Game."* (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1971. Pp. xii, 578. \$12.95.

This work—a reference library packed into one volume—is crammed with events, names, and dates covering some 750 years of Chinese, Russian, and Central Asian history. Clubb's knowledge and research of so broad an area is admirable. Yet while working oneself through more than 550 pages of text and notes—and real work it is—the thought occurs whether it is feasible to cover so many centuries and so vast an area in one volume. I believe not, despite Clubb's heroic effort. Some aspects should have been sacrificed; this book, valuable as it is for historians and political scientists alike, lacks analysis in depth, especially of the modern and the contemporary eras. The task of a historian goes beyond that of a chronicler. He must explain at length the meaning of dates and characters during a specific stage of history in terms of the main topic: first, relations between China and Russia, and later on, Sino-Soviet relations. Clubb sometimes attempts to do so, but whenever he has valuable insights, more often than not he buries them under an avalanche of facts.

No reviewer, try as he may, can be absolutely objective. He can perhaps suppress his predilections but not his experience. On this basis, I cannot agree with some of the author's views. For example, Clubb should have been rougher on Chiang Kai-shek's behavior toward General Stilwell and far rougher on Patrick Hurley and the policy makers whom he duped in 1942. I thought that Barbara Tuchman's book on Stilwell had a more realistic approach. I mention this because the mistakes made by both Chiang and Washington had dire consequences. Hurley's lack of comprehension, in particular, prevented us from an earlier understanding of the Chinese Communists. Whether the men in Peking should be "viewed as communists—but



first as Chinese" needs far more analysis than Clubb has space for. A similar question once was posed with regard to Poland: was Gomulka first a Pole and then a Communist? I submit that Mao and Gomulka are both Nationalist and Communist but that their lifelong dedication to Marxism-Leninism and their revolutionary experience would influence their decision. Nevertheless, if the leaders of a Communist state find that their national interests have to be safeguarded (as happened increasingly during the sixties and early seventies), the founding fathers of communism may color their approaches but no longer decisively direct their policies—at least temporarily. I doubt that Mao's 1957 epigram "the eastwind is prevailing over the westwind" has any meaning left at all. It needed only fifteen years for this slogan to deteriorate and presumably be buried in the graveyard of discarded Communist theses.

Perhaps Clubb's brief epilogue ("Chinese, Russians and Americans") contains the most fruitful pages in terms of a contemporary estimate of world politics. It is evident that Clubb is more familiar with China than with the Soviet Union, but his outline of the great power chess game being played by Washington, Moscow, Peking, Tokyo, and New Delhi is interestingly put. I wonder how he would interpret the American rapprochement with Moscow and Peking, which occurred after he had completed his manuscript.

To recapitulate: the book is a mine of information for researchers and scholars in various fields of study throughout Eurasia. There is an overflow of material on countries, personalities, dates, and politico-military developments. The growth of China and Russia from relatively small boundaries to their present immense size could have been pictured more clearly without the ballast of too many details. The analysis of developments between 1223 and 1970 is too scanty, but perhaps understandable in a work of such scope. Clubb's command of English is as spectacular as the wealth of his research. All in all, this is an indispensable book for libraries, historians, and international affairs specialists.

K. L. LONDON

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MARK MANCALL. *Russia and China: Their Diplomatic Relations to 1728*. (Harvard East Asian Series 61.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xii, 396. \$12.00.

The book's jacket, foreword, introduction, and earlier reviews all emphasize the relevance of this study to a contemporary understanding of the problems involved in Sino-Soviet and Sino-Western relations. This question of relevance is currently rather fashionable, but is beginning to become tiresome. Needless to say, books that assist in understanding Chinese thought and action are extremely useful in this period of the Brezhnev doctrine and the Nixon détente. What should be stressed as important about this volume, however, is that Professor Mancall has produced a first-class piece of historical writing. It needs no further explanation, justification, or relevance.

This work deals with Sino-Russian contacts and relations from the mid-seventeenth century when Cossack explorers first arrived along the Amur River to the year 1727 when the treaty of Kyakhta was signed. Emphasis naturally is placed on the background negotiations leading to the compromise settlement of problems in the Nerchinsk and Kyakhta treaties, and on the semicommercial, semidiplomatic Russian caravans sent to Peking during the interim period, 1689–1727. The point is clearly made that while Russians regarded their contacts with China primarily as a commercial venture, the Manchu government was mainly concerned with fitting the Russians into what is commonly known as the traditional tribute system by which China dealt with, and controlled, the peoples on its inner-Asian frontiers. Thus from the earliest stages, relations between the two empires were bedeviled by conflicting and mutually incomprehensible East-West, Confucian-Christian thought patterns. That the Russians adapted themselves to China's system of barbarian relations is testimony both to their desire for trade and profits and to the Manchu's position of political and military strength vis-à-vis Russia's position of weakness along the Amur.

In an epilogue, Professor Mancall shows that in the nineteenth century, when new problems arose in Sino-Russian relations after over a century of stability, the situation was not simply a case of Russian strength and aggressiveness ver-

sus Chinese weakness. An entirely new and different European outlook on international relations had developed. No longer could the civilized European adapt to China's world view, but instead China must be forced to adapt to the system of relations between nation-states created in late eighteenth- and early nineteenth-century Europe. China's inability to so adapt brought it to the brink of almost complete disaster.

That this study is based on extensive, multilingual research is indicated by a detailed bibliography and no less than sixty-one pages of footnote references. An appendix of Russian-Chinese treaties, a glossary of Chinese-Manchu terms, and an adequate index absorb another forty pages to complement a somewhat slim text of 276 pages. Three maps are provided, but are of dubious value.

As so often happens when one finishes reading a well-written history book, there is an ambivalent feeling of satisfaction and deprivation. What Professor Mancall has provided is good, but with all the research done, the reader wishes that more had been said. Numerous areas could have been profitably expanded. The pivotal role of the Mongols in influencing both the Russian and Manchu positions at Nerchinsk is but one topic where more information would have been welcomed. Nevertheless, this study will undoubtedly remain as a standard reference for many years to come, and is highly recommended to all students of Russia's tortuous relations with the Middle Kingdom.

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S. F. ORESHKOVA. *Russko-turetskie otnoshennii v nachale XVIII v.* [Russian-Turkish Relations at the Beginning of the 18th Century]. (Akademiiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 203.

Svetlana Oreshkova has avoided upsetting Russian national sensitivities and tendencies for hero worship in her book on Peter the Great's near-disaster, the Pruth campaign of 1710-11. Following the Russian triumph over the Swedes at Poltava, Charles XII sought refuge on Turkish territory. His presence there caused the stationing of Russian forces in Poland to

interdict the escape route, encouraged French diplomatic pressures upon Constantinople, and incited the Crimean Tatars in favor of a war of revenge—the chief factors in the Ottoman decision to commence hostilities with Russia. Peter then copied the mistake of Charles XII by launching a small, inadequately supplied army, with himself in it, deep into Turkish Balkan territory, where it was quickly surrounded and defeated. Unlike Charles, however, the tsar could not escape and was forced to sue for peace.

This book concentrates on relating and analyzing the complicated diplomatic relations before, during, and after the campaign up to the signing of a new peace in 1713. On the whole it is a valuable contribution to an understanding of previously neglected aspects of international relations during the War of the Spanish Succession and the Great Northern War. Utilizing most of the available Russian and Turkish published sources, Oreshkova emphasizes the role of the French in creating a "diversion" for the benefit of their defeated Swedish ally. She neglects, on the other hand, a thorough treatment of the border nations, especially Ukrainian Cossacks and Crimean Tatars, who were also involved in the conflict.

Peter the Great is strangely and artificially relegated to the background, while Charles XII and the Turkish officials debate strategy and the Russian generals make the best they can of a bad situation. According to Oreshkova, conflicts between Turkish, Swedish, and Tatar objectives and the precarious circumstances of Ottoman mobilization and supply made it possible for the Russians to come out with better terms than might have been expected. She, perhaps too readily, dismisses the influence of Russian bribes on the Ottoman leaders.

With an emphasis on diplomatic detail, Oreshkova's work is difficult to compare with the only book in English devoted to the subject, B. H. Sumner's *Peter the Great and the Ottoman Empire* (1949). While Sumner emphasizes the personal role of Peter and how he was influenced by background situations in the area, particularly by the problems of Balkan Christians, Oreshkova practically ignores religious and nationality issues.

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ALAN LLOYD. *The King Who Lost America: A Portrait of the Life and Times of George III*. Garden City, N.Y.: Doubleday and Company. 1971. Pp. x, 369. \$7.95.

STANLEY AYLING. *George the Third*. New York: Alfred A. Knopf. 1972. Pp. 510. \$12.50.

JOHN BROOKE. *King George III*. With a foreword by H.R.H. THE PRINCE OF WALES. New York: McGraw-Hill Book Company. 1972. Pp. xix, 411. \$12.50.

The bicentennial tide begins to run on the eastern side of the Atlantic. Here are three English biographies of George III, America's last crowned monarch who must always occupy a central position in our national history and myth. Each study shows the marked influence of Sir Lewis Namier and provides a sympathetic interpretation of the king whose reign from 1760 to 1820 comprised much more, we are reminded, than the American secession from the First British Empire.

Mr. Lloyd's book does not pretend to scholarship. It is a "portrait of the life and times of George III; a highly entertaining portrait of the rather endearing prig who lost the colonies." It need detain us no longer.

Messrs. Ayling and Brooke have a legitimate and strong claim on our attention. Neither has produced a "definitive biography, but both have written useful and important books. Mr. Brooke, senior editor of the Royal Commission on Historical Manuscripts and Namier's former student and collaborator, contributes a volume, embellished with mild grace by Charles, Prince of Wales, to the McGraw-Hill American Revolution Bicentennial Program. Mr. Ayling, too, is an experienced student of the eighteenth century and has a professional command of the sources. His use of secondary works would appear to surpass Mr. Brooke's. Although both interpretations are in close agreement, Brooke explores constitutional and political problems in greater depth; Mr. Ayling provides a better treatment of the king as family man, patron, and book collector. Each accepts porphyria as the cause of the king's mental derangement. Mr. Ayling must be allowed the edge in style since he avoids both far-fetched parallels with twentieth-century events and personalities, and the temptation to pontificate.

Forty years ago Namier's pioneer studies in-

dicated the need for a systematic re-evaluation of King George III; but, while many aspects of eighteenth-century British politics have subsequently undergone Namierite revision, the king has remained in the garish and unflattering light shed by the Whig historians. The *mise en scène* arranged by these writers contains elements from malicious and unreliable Horace Walpole; from Scot-hating and self-loving John Wilkes and his friends; from Edmund Burke's faulty and obsessive perception of a double ministry—one legitimate, public, and dominated, of course, by his friends the Old Whigs, the other secret, irresponsible, and dedicated to royal absolutism; from the celebrated indictment in the Declaration of Independence; from Charles Fox whose personal antipathy for the king was unbounded and unending. Here are the sources of the informing spirit of works by Macaulay, Lecky, Trevelyan, and the countless multitudes who followed where they led. In 1890 a kind of historical dogma was proclaimed with the king's entry in the *Dictionary of National Biography*. Excoriated by apologists for the American Revolution, the luckless monarch also receives the lash from imperialists who punish him for having lost the colonies. George III has indeed received the worst of several worlds.

Brooke and Ayling at last give the king much justice. Drawing deeply on original sources, they succeed in overturning the legend of the obtuse, mulish (the word is Jefferson's) absolutist foiled, to his country's salvation, by American revolutionaries and his own timely insanity. The facts adduced are clear: the prince received no Tory education from his mother, Bute, or anybody else, least of all from his impeccably whiggish but hopelessly inept tutors and preceptors. Brooke's analysis of George's schoolboy essays puts the matter out of court. The king aimed at no restoration of Stuart monarchy. His pattern was William III. He planned no deep change in Britain's political system, domestic or imperial, although changes, *malgré lui*, were at work. What he inherited, he venerated and strove mightily to preserve. A conservative, he aimed to enslave no man; but he himself was obsessed with the fear that unscrupulous men aimed to dominate him and thus to subvert balanced, constitutional government. Brave, honorable, and reli-

gious as a private person, George III personified those virtues most highly prized by the English middle class which came to revere and to love him. Despising the social *haute monde*, where his son and heir was in his swinish element, this king was the most whiggish and decent of the Hanoverians. So much Brooke and Ayling have put beyond serious question; theirs is no mean accomplishment.

If they share equal credit, they also bear equal fault. *Demolition* is more complete than reconstruction. Neither replaces satisfactorily the interpretation of the Whigs. The obverse of the coin is displayed, but the figure is idealized and in profile. We have yet to see the king in the round; while earlier writers may have magnified the royal blemishes beyond reason, they tend here to disappear altogether. No manner of historical rationalization can conceal the fact that many of the "discontents" of the king's long reign arose from a remarkable inability to adapt to change, and some of them at any rate from actions that can only be judged manifest violations of established constitutional doctrine.

These books are nonetheless stout blows struck in a worthy cause. One is tempted to employ Mr. Brooke's favorite literary device, however, to say that they mark the end of a long *Sitzkrieg*. Many bloody campaigns remain to be fought. It should be an exciting war.

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RAPHAEL MAHLER. *A History of Modern Jewry, 1780-1815*. New York: Schocken Books. 1971. Pp. xxiii, 742. \$15.00.

Students of modern Jewish history will welcome the appearance of this volume, a translation and condensation of the four volumes of the monumental Hebrew original. It makes available to a wide circle of English readers the first part of the culminating work of a distinguished representative of the school of Jewish historians in interwar Poland. At the same time, this volume and the ones to follow will go a long way toward filling the great need for a re-evaluation of modern Jewish history as a whole, a field that has been rather poorly served, despite a wealth of new developments and new knowledge since the days of Dubnow and Graetz.

Professor Mahler's history is impressive in its scope and erudition. It deals in rich detail and often with profound insight not only with every major and several minor European Jewish communities in this period, but with the United States and Palestine as well. More important still, a significant place in each chapter is devoted to a systematic analysis of the economic life of particular communities, including their social structure and the class tensions and struggles within them. I know of no other recent work (except Professor Mahler's own two volumes in Yiddish, *Jews in 18th Century Poland in the Light of Statistics* [1958]) where such a wealth of documentation is to be found.

Intellectual developments are not neglected, and perhaps the most interesting chapter of all is that devoted to Hasidism and its opponents in Eastern Europe. This is not unfamiliar ground, to be sure, but it is luminously treated here with rare sensitivity and insight. In general the vitality of East European Jewry emerges in all its vigor and color from Professor Mahler's pages. Polish Jews were secure in their identity despite their precarious and often desperate economic and political condition (which non-Jewish "reformers" were constantly making worse in the name of making it better). By contrast the fallen state of Western and Central European Jewry makes painful reading. Under the Old Regime the cultural unity of Ashkenazic or Yiddish-speaking Jewry was largely unbroken from the Seine to the Dnieper. But precisely in the relatively advanced countries like France and Germany the legal and economic status of the Jews was particularly low and distorted, and their cultural life (except briefly in Prussia) was more barren than in the East. Professor Mahler makes clear the devastating impact on these communities of the revolutionary changes of the late eighteenth and early nineteenth centuries, as Jews struggled awkwardly to integrate into a new society that (in theory at least) welcomed them as men, but rejected them as Jews. Professor Mahler's condemnation of assimilationists like the members of the Assembly of Notables and the *Grand Sanhedrin* in Napoleonic France is perhaps unduly harsh, but the inevitable decline of traditional Jewish life in the age of nationalism and the nation-state is poignantly evoked and in general handled with understanding.

It is no easy task to write Jewish history in all its diversity and to strike a balance between internal developments and a welter of external influences. But Professor Mahler has succeeded magnificently. One might regret the unaccountably sketchy treatment of British Jewry or question whether nearly one hundred pages on Palestine and early schemes for the restoration of a Jewish state there are really justified. In particular the elimination (no doubt for reasons of cost) from the otherwise full bibliography of all books and articles in Hebrew and Yiddish is to be regretted. But these are relatively minor points. This volume will undoubtedly be the standard work in its field for years to come, and the next volume and its successors will be eagerly awaited. I wish Professor Mahler the strength and long life needed to complete the great task he has begun.

SOLON BEINFELD  
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AKIRA IRIYE. *Pacific Estrangement: Japanese and American Expansion, 1897-1911*. (Harvard Studies in American-East Asian Relations, 2.) Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 290. \$12.00.

This extraordinary book adds a new dimension to our understanding of American-East Asian relations in the late nineteenth and early twentieth centuries. Moving beyond the traditional emphasis on diplomacy, politics, and strategy, Akira Iriye places the encounter between Japan and the United States in a psychological and cultural framework. The result is a subtle, penetrating study that analyzes for the first time the complex interaction between Japanese and American expansionism.

Iriye describes the emergence in Japan of a dynamic expansionism, one strain of which emphasized predominance in Korea and spheres of influence in China, another the flow of emigrants and trade across the Pacific. It seemed essential for a vigorous, growing nation to spread its people and culture over a wide area of the globe. Peaceful expansion would relieve population pressures and enrich the nation through the creation of Japanese colonies abroad. Many Japanese believed that, unlike the Chinese, their settlers would be welcomed in California, Latin America, and the islands of the Pacific. Japanese expansionist literature

dwelled on California's abundance and mild climate. Expansion on the continent of Asia was essential for national security, but the American Pacific Coast seemed far more promising as an area to settle.

As America's empire spread into the Pacific, tension with Japan increased, fed by a powerful current of domestic racism. By 1905 the specter of a yellow peril pervaded all strata of American society, and, as Iriye shows, affected American policy makers and army and navy strategists. These racial fears gave the Japanese-American crisis over immigration a special meaning for American leaders and endowed their policy with great urgency. Theodore Roosevelt, torn between his fears of estrangement and his hopes for amity, encouraged Japanese expansionists to turn toward continental Asia, where the United States presumably had no vital interests. The Japanese government, stunned by the racial animosity in the United States and unable to grasp its full import, moved reluctantly in this direction, aided by a gradual realization of the opportunities that lay in southern Manchuria.

Parallel with this development, however, came the emergence of a new American interest in China and a belief, transformed into policy under William Howard Taft, that the United States had a special mission in that nation. Japan resisted American attempts to penetrate its sphere of influence, and slowly the dominant image in Japan of a peaceful intermingling of the two peoples gave way to the conviction of a profound antagonism. By 1911 it was difficult for men on both sides of the Pacific to conceive of a harmonious relationship between their two nations. Instead the images of conflict and strife so deeply embedded in the American official and popular imagination became embedded in Japan as well. They would persist and contribute to the inability of both governments to comprehend the dilemmas and aspirations of one another.

Iriye explores with freshness and insight the patterns of estrangement revealed in the writings of American expansionists and the calculations of government leaders. He also opens a door into a fascinating world of Japanese expansionist literature. The relationship of this body of thought to the Japanese ruling oligarchy is not always clear, but Iriye is more inter-



ested in intellectual currents than in the structure of decision making within Japan. Thus his book complements Shumpei Okamoto's fine study, *The Japanese Oligarchy and the Russo-Japanese War* (1971). It is a landmark, transforming the foreign policy of the period by placing it in a broader framework and providing one more example of the vitality of the whole field of American-East Asian relations.

CHARLES E. NEU  
Brown University

R. SH. GANELIN. *Rossia i SShA, 1914-1917: Ocherki istorii russko-amerikanskikh otnoshenii* [Russia and the U.S.A., 1914-1917: Essays on the History of Russian-American Relations]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka," Leningradskoe Otdelenie. 1969. Pp. 416.

Rafail Ganelin's "essays" on Russia and America in 1914-17 heavily stress financial and economic relations. About one-third of the book covers the period down to the February Revolution; its bulk deals with the Provisional Government.

A detailed and professional study, the book is relatively restrained and matter-of-fact. If at times it seems wooden and petty, it is distinctly less primitive than earlier Soviet treatments of the same period. While Ganelin provides ample detail on the dealings of American promoters, banking representatives, and business agents with Russian officials and companies, he recognizes that, however "incriminating," this aspect does not adequately explain the course of Russo-American relations: political and military circumstances loom large, though with regard to these his treatment is far less adequate.

The author omits various aspects of diplomatic exchanges (e.g., concerning the Far East). He alludes only casually to prior impediments to trade (e.g., Russian anti-Semitism). There are interesting but inadequate glimpses of broader political visions, such as Russian hopes to make the United States a counterweight to Anglo-French influence.

Like others, Ganelin abandons the earlier Soviet formula that Russia had become a "semi-colony" of Western imperialism. But ambiguities remain, such as those regarding the extent of Allied pressure on the Provisional

Government. It also remains unclear in what measure business interests influenced Wilson's and Lansing's policies. Ganelin detects in American diplomacy "the absence of a firm tactical line and of coordinated action" and points up the inherent conflict—and U.S. failure to make clear choices—among military objectives (keeping Russia in the war, once the United States had joined in), politico-ideological goals (forestalling a Bolshevik take-over, even at the price of Russia's leaving the war), and the more obscure plans for American post-war economic penetration. His account reveals a familiar syndrome of American behavior—meddling, naiveté, gullibility, condescension, moralizing, profit making, and missionary zeal. Predictably, he is even less charitable in delineating the Russian protagonists of 1917.

Ganelin cites unpublished documents from Soviet archives, particularly on economic and financial dealings. His use of diplomatic archives is less thorough. He uses standard U.S. sources and relies heavily on a handful of secondary American accounts. But he fails to use a variety of memoirs and monographs that might have amplified or corrected some of his account.

All in all, though far from a definitive study, the book has some interesting and novel material. While its treatment deserves to be taken with skepticism, it is modestly encouraging as an indication of serious Soviet research in a relatively sensitive field.

ALEXANDER DALLIN  
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KLAUS SCHWABE. *Deutsche Revolution und Wilson-Frieden: Die amerikanische und deutsche Friedensstrategie zwischen Ideologie und Machtpolitik 1918/19*. Düsseldorf: Droste Verlag. 1971. Pp. 711. DM 87.

The story of the Armistice and of the Versailles Conference has been told in detail before, but this massive *Habilitationsschrift* by an instructor at Freiburg University succeeds in making a distinctive and valuable contribution by narrowing the focus to the field of German-American relations and by assiduously mining hitherto neglected American and German sources.

Schwabe rejects the notion that the "idealist" Wilson was outmaneuvered by "realists" like Clemenceau and Lloyd George and that he



therefore betrayed both Germany and his own principles at Versailles. While conceding that the final settlement did not entirely conform to the president's stated aims, he contends that the modifications were a conscious result of Wilsonian realism and clashed more with the illusions entertained by the Germans and by "Wilsonians" elsewhere than they did with the beliefs of the president himself.

Wilson regarded the territorial settlement with Germany as conforming to the Fourteen Points and resented German attempts to deny this. He clearly believed in German war guilt from the beginning and was never convinced that that nation had been sufficiently reformed. He therefore saw neither injustice nor a deviation from principle in the provision for war crimes trials or the postponement of German membership in the League, and looked askance at the efforts of German spokesmen to defend national honor by denying culpability.

Only on the question of reparations did Wilson consciously retreat, and he did so for the most practical of reasons. Without some compromise on this issue the peace conference would undoubtedly have broken up. To Wilson that would have meant a resounding personal defeat, with catastrophic political repercussions at home. It would also, in all likelihood, have brought more rightist governments to power in Britain and France, led to a resumption of hostilities, and made far more difficult the achievement of the stable world order that Wilson sought. To risk all of this either in the name of abstract principle or in the interest of a defeated enemy was never a realistic alternative, and that fact clearly limited Wilson's room for maneuvering.

The Germans, in their dealings with Wilson, were profoundly mistaken on two counts. They failed to understand that the president's concept of justice included the belief that nations must accept the consequences of their wrongful acts and could not expect to escape these by protestations of innocence or appeals to a morality they had violated. Moreover, they grossly misread the effect of Allied disunity. Far from allowing them to play off America against the French and British, that very disunity forced Wilson, as the man most sincerely committed to peace and order, to make concessions.

Schwabe's analysis emphasizes the pragmatic

character of Wilson's "idealism" and reveals the president as an essentially conservative statesman whose new international order was far more traditional than is often assumed. It also throws serious doubt on recent interpretations that have stressed the anti-Bolshevik element in Wilson's policies. The fear of a "bolshhevized" Germany in the heart of Europe seems to have affected the president only for a brief time after the Armistice. Thereafter he consistently failed to respond to German appeals for support on that basis, and he mistrusted the Ebert government, despite its anti-communist character, on the grounds that it contained right-wing and militarist elements.

Schwabe has uncovered a wealth of evidence and has succeeded in delineating the complexity of the situation both for the German government and for the United States. He is particularly good at defining the communications problem and in describing the activities of various middlemen such as McNally, Herron, and Conger on the American side and Hahn and Loeb on the German.

There are some weaknesses. Schwabe at times gets bogged down in details, and his speculations on occasion go substantially beyond what the evidence will support. While thankfully avoiding the use of impenetrable German academese, his attempts to maintain readability are marred by lapses into stylistic cuteness and by his over-reliance on exclamation points to provide emphasis. This is, however, a solid contribution to the literature and deserves the attention of all serious scholars.

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SIR JOHN WHEELER-BENNETT and ANTHONY NICHOLLS. *The Semblance of Peace: The Political Settlement after the Second World War*. [New York:] St. Martin's Press. 1972. Pp. xiv, 878. \$35.00.

This massive work is a splendid example of history written in the traditional vein and the grand manner. "It has been our aim," say the authors, "to illuminate the origins and significance of the uneasy and interrupted peace which followed the defeat of the Axis powers"; they have chosen as both epigraph and conclusion a somber passage from W. B. Yeats: "Civilization is hooped together, brought / Under a

rule, under the semblance of peace,/ By manifold illusion." As the sonorous prose rolls majestically on, it carries overtones of Shakespearean or even Greek tragedy: in a sequence of scenes exotically staged all the way from Newfoundland to Tehran, the protagonists appear and reappear as embodiments of wisdom or folly, visionary idealism or calculating *Realpolitik*; they seem to be wrestling with destiny under a lowering lead-gray sky, with a hint of mushroom cloud on the horizon. The reader finds himself caught up in the mood and almost unwittingly tempted to imitate the style.

It would be unfair to expect any startling revelations of fact in a book on this subject. The era of transition from hot to cold war has been a historians' battleground for more than a decade, and the available sources have already been combed over in microscopic detail. Indeed, for American New Left historians, explicating the origins of the cold war has become almost a major industry, or a theological testing-ground for separating Truth from Error. Time was when the origins of the 1914 war served that same purpose. Then as now, the revisionists at a certain point carried the day, and their thesis crystallized for a time into a kind of new orthodoxy. Perhaps one should see *The Semblance of Peace* as a major manifesto of counterrevisionism, heralding a new phase in the battle about the cold war.

This is not to say that the authors adopt an openly contentious stance, throwing down the gage of battle to the revisionists and challenging their interpretations point by point. Indeed, they rarely confront the revisionist view directly, but mention it mainly in brief footnote references, rather as though one of Admiral Fisher's dreadnoughts found itself steaming majestically past a PT boat. Nevertheless, they make no bones about their own position. "Between our views and those of the Revisionists there is clearly a stark contrast which is irreconcilable." Soviet foreign policy "was a re-embodiment of those imperial ambitions which in the glory of the Tsarist expansionist period" had led to a series of aggressive acts and wars. By mid-1945 "it was apparent that Stalin's ambitions in Europe and in Asia even surpassed the paranoiac schemings of the most extreme fanatics" of tsarist days. Therefore "the West-

ern Powers were constrained to accept the Soviet challenge and to make a brave and essential response of free men against aggression."

Such a view, bluntly and baldly stated, has become so unfashionable in recent years as to seem almost a novelty (at least in academic circles). No doubt it will be brushed off by many critics as both wrong-headed and terribly dated—as nothing more than an artifact left over from the cold war era itself. Possibly it is. Yet surely it must be taken more seriously than that. The authors have, after all, built their case on exactly the same body of evidence used by the revisionists. We may assume (pending proof to the contrary, anyway) that neither side has consciously warped or twisted that evidence to support a preconceived thesis, and that the contradictory readings represent honest differences of judgment, rooted no doubt in different value-systems or deeply felt patterns of prejudice. The conclusion that seems to emerge is that the available evidence does not establish, beyond the shadow of a doubt, the validity of either the revisionist or the counterrevisionist case.

Such a conclusion is hardly surprising, given the complexity of the historian's enterprise in seeking to reconstruct past events and to assess human motives. What is surprising is that historians of both schools have thought themselves able to arrive at such certainty of judgment in the absence of really dependable evidence about how the world looked from the Kremlin. Even with an assist from Khrushchev, we have precious little basis on which to judge the real motives and intentions of the Stalin crowd. Something can of course be inferred from their actual conduct; but we have nothing remotely like the inside accounts of evolving attitudes and clashes of opinion in London and Washington. Which Western leader or faction at the time guessed right about the aims of the enigmatic figure in Moscow? Does any present-day historian really have much more to go on than Stalin's contemporaries did? This, it seems to me, is the most important flaw in the case constructed by Sir John Wheeler-Bennett and Anthony Nicholls. But it is exactly the same flaw that mars the revisionist case as well. Both sides hypothesize a Stalin (and a Stalinist entourage) that may or may not have ex-

isted in fact. Yet both sides are quick to chastise certain Western statesmen for having done the same thing twenty-five years ago.

Sir John and Mr. Nicholls are especially hard on Franklin Roosevelt for what they see as his naive illusions about Stalin and his self-confident belief that he alone could manage Uncle Joe; they even find a parallel with Chamberlain confronting Hitler. But Roosevelt, as they view him, was not merely naive; he was also a shrewd, calculating political leader whose aim was to superimpose upon the United Nations "an American-Soviet alliance, which should dominate world affairs to the detriment of Britain and France." Roosevelt's abiding suspicion of British aims was shared, the authors contend, by a number of his close associates. Indeed, this theme of British-American rivalry for Stalin's favors recurs throughout much of the book as a kind of counterpoint to the main theme. It is not surprising, therefore, that most of the heroes are British: above all, the tough-minded and farsighted Anthony Eden, portrayed as a master diplomatist and a tireless defender of British and European interests. At one point Eden is applauded for his refusal to budge on an issue just "to suit the parochial prejudices of Mid-Western Americans." Not all of the Americans are fools or villains, but the chief offenders among them are scourged without mercy: notably Hull, Davies, Leahy, and above all James Byrnes. Harry Truman, on the other hand, comes off well; he "personified that combination of fundamental toughness, commonsense and goodness of heart which comprises the average American." Besides, Truman in 1946 put an end to what is described as Byrnes's "whole-hearted appeasement of Moscow," and went on to promulgate the doctrine that "was to save Western Europe . . . from Soviet annexation." If Sir John and his colleague are inclined at times to speak with a John Bullish voice, they do perform a useful service in reminding readers that East-West tensions were not the only ones that shaped the era.

Sir John and Mr. Nicholls also give us masterful accounts of the successive wartime and postwar allied conferences, of the planning and conduct of the war crimes trials, and of the complex negotiations that finally led to peace

treaties with each of the defeated enemy nations except Germany. The story of the Nuremberg Trial is enlivened by Sir John's first-hand notes (he was attached to the British prosecution)—though once again it is the British who quietly triumph when the American Justice Robert Jackson has to be "rescued" from his blunders by Sir David Maxwell Fyfe.

Despite its thorough and meticulous documentation, the book now and again leaves the reader unsatisfied. We are told, for example, that Stalin in July 1944 almost certainly expected to pull Greece into the Soviet orbit. If so, why his consistent hands-off policy in the months that followed? We are told that when Stalin described Chiang Kai-shek in 1945 as China's best hope, he was "playing the greatest game of bluff and deception of even his remarkably devious career." But what if Stalin was, once again, simply ill-informed about China? We are told that the disarray of the Grand Alliance just after V-E Day was caused by "the altered policy of the Soviet Union," yet only a few pages later Churchill is praised for his early awareness of the unchanging nature of Soviet policy. In the chapter on Yalta there is only the sketchiest reference to the Declaration on Liberated Europe, even though some historians have argued that its importance was central both to the Yalta settlement and to the misunderstandings that followed. Nor do the editors really grapple with the argument of some revisionists that Soviet unilateralism in occupied Romania and Bulgaria was only a natural response to the West's cold-shouldering of the Russians in the occupation of Italy. As for the domestic roots of foreign policy, no attempt is made to probe that vast and difficult subject.

A monument, even when flawed, remains a monument. *The Semblance of Peace*, like the Great Pyramid, will surely loom up for many years to come on the horizon of historical scholarship.

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P. J. VATIKIOTIS, editor. *Revolution in the Middle East: And Other Case Studies*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 231. \$10.00.

DAVID C. GORDON. *Self-Determination and History in the Third World*. Princeton: Princeton University Press. 1971. Pp. 219. \$7.50.

More than a decade has passed since third world countries gained their independence, and observers are now anxious to assess the significance of these movements. How much did the struggles for national independence change societies? How truly revolutionary were the policies of leaders like Nasser and Nkrumah? In general it is fair to say that the original enthusiasm that greeted the rise of nationalism and decolonization has given way to disillusionment and disappointment at the progress toward political unity and economic development. Decolonization has not seemed revolutionary. The nationalist movements did not transform their societies. Whereas books in the 1950s were full of praise for third world nationalism, a scant decade later Frantz Fanon's searing neocolonialist critique, *Wretched of the Earth*, was being read as descriptive of the plight of many newly independent states.

Readers will approach the collection of essays on revolution in the Middle East with considerable interest. Such an eminent group of scholars should shed light on the vexed question of whether there have been major revolutions in African and Asian countries, especially during the period of decolonization. But one's hopes are disappointed. To be sure, the essays underscore the prevailing unhappiness about the performance of contemporary Middle Eastern economies and politics. Dr. Roger Owen assesses the economic change that followed political revolution in Egypt, Syria, and Iraq, and while he finds "respectable" economic performance, he feels that fundamental economic and political change has not proceeded rapidly enough. Albert Hourani argues that there was no true revolutionary tradition in the Ottoman Middle East until after British and French withdrawal. Earlier upheavals, which were often called revolutions, did not produce major structural political and social change and sought only a realignment of political forces within the existing framework.

Nevertheless, the reason these essays fail is that they are short and superficial and do not take sufficient account of important works on the French, Russian, and Chinese revolutions that have decisively shaped contemporary ideas

about revolutions. Albert Hourani's use of the *Shorter Oxford English Dictionary* definition of revolution as the "complete overthrow of the established government" neglects the social and economic dimensions of revolutions that are so important in modern scholarship. In a weak and largely unsuccessful effort to create a framework for viewing revolution, the book contains two introductory essays by A. T. Hatto and Bernard Lewis on the semantics of the word "revolution" in the West and Islam. But these statements give no insight into the way contemporary scholars use the word. Would it not have been helpful to have had a historiographical essay on revolution? The best discussion is the longest and most substantial, an analysis of change and continuity in Morocco, Algeria, and Tunisia. Its authors (R. Le Tourneau, M. Flory, and R. Duchac) chart the transformations in these North African societies and offer explanations of the varying patterns. One of their most interesting ideas is that the Algerian revolution began to decelerate when the leadership became fragmented and lost its capacity to mobilize the masses.

David Gordon's volume, *Self-Determination and History in the Third World*, discusses the important subject of the transformation of third world history in the last few decades. The colonial era left a historical legacy in Asia and Africa of inferiority, backwardness, even historylessness. Gordon catalogs the reassertion of the dignity and worth of Asian and African history, an intellectual movement with strong roots among Western scholars as well as in the third world. The purview of the essay is broad; Gordon proposes to consider historical writing about the "Arab states of the Middle East and North Africa, India and Greece to a limited extent, some African states, Turkey, and the United States with respect to the Black Nationalist movement." In fact the scope is much too broad for the author's expertise. Gordon is comfortable dealing with North Africa, and his next to last chapter, "The 'Copernican' Revolution," is the most thoughtful and thoroughly researched. Here he shows how seven major themes of North African history have undergone reinterpretation as part of a movement to decolonize history. In this section the author has command of his sources and knows the views of historians, journalists, and other intellectuals.

The same cannot be said for his excursions into other third world historiography where his knowledge is much more sparse. In dealing with black Africa his only reference to a Nigerian historian is K. O. Dike, admittedly one of the pioneer African historians. But he does not seem to have consulted the writings of other Ghanaian, Nigerian, and African historians. He also comes to grief trying to define historical maturity, arguing that this is revealed when a society learns to view the colonial period "realistically as a period which, with all its bitter memories and frustrations, nevertheless has become a part of [a country's] heritage and has contributed to her modernization." Is it true then, that the colonial experience was the same for everyone? Will the Angolans and the people of Mozambique attain historical maturity only when they see the colonial period as one that brought modernizing benefits? Unfortunately much of the book is full of such quick and superficial judgments.

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IRENE L. GENDZIER. *Frantz Fanon: A Critical Study*. New York: Pantheon Books. 1973. Pp. xvi, 300. \$10.00.

Dr. Gendzier has added significantly, in this work, to the limited body of materials by and about Fanon. The work is extremely well designed and most carefully researched. Furthermore, she has not allowed herself to fall under the mystic spell of the West Indian psychiatrist and the revolutionary philosopher. For the student, she has also provided both a comparison between her findings and views and those of others, such as Caste, Geismar, and Zahar, and a guide for further study. This later section is an unusual section in such a work.

The work is divided into four sections: "The Search for Roots, 1925-1952," "Toward a Psychology of Colonial Relationships, 1953-1959," "The Militant, 1956-1961," and "The Summing up: *The Wretched of the Earth*." In presenting Fanon's background the author presents Fanon's dilemma between his self-image and the image others had of him. Fanon was intrigued not only by his own attitudes on blackness, but the whites' consciousness of

blackness. Negritude early attracted the attention of Fanon, although in his mature years he saw it a means toward an end and not as a national ideology. The first section closes with a study of Fanon's first work, *Peau noire, masques blanc* (1952), translated into English as *Black Skin, White Masks* in 1967. This book deals largely with Fanon's Martiniquean experience—"the fact of blackness and the education to 'whiteness.'" Fanon deals with the French language as an instrument of assimilation in a most interesting manner.

The second section of the work deals with Fanon the psychiatrist through his experience at St. Albans and his time as "chef de service" at Blida-Janville in Algeria. At Blida, Fanon dealt with Muslims, people of a different cultural background, and found that the psychiatry of France did not solve the problems of his Muslim patients. During his stay at Blida he became enmeshed in the whole subject of colonialism and the rising tension of Algerian-French relations. He identified increasingly with the resistance movement in Algeria.

The third section, the heart of the study, deals with Fanon's years as a militant, 1956-61. This period saw Fanon's development as a political thinker. He was active as a writer for *el Moudjahid* between September 1957 and January 1960. In his writing of this period he concentrated on four main themes: French colonialism in Algeria, the response of the French Left, including the Communist party, to the Algerian question, the position of the FLN on minorities, and relations between developments in Algeria and the Maghrib and Africa.

The last section deals with Fanon's thoughts on violence, the role of the peasants in the revolution of spontaneity against organization, the political parties and the nation, and national culture. A chapter deals with Fanon's death and his influence on the Algerian revolution. Dr. Gendzier deals realistically with Fanon's long-range influence both on Algeria and Africa and the whole spectrum of human development.

This may well be the definitive study, at least for the time, on Frantz Fanon.

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## ANCIENT

WILLIAM A. McDONALD and GEORGE R. RAPP, JR., editors. *The Minnesota Messenia Expedition: Reconstructing a Bronze Age Regional Environment*. Minneapolis: University of Minnesota Press. 1972. Pp. xviii, 338. \$22.50.

In 1939 the late Carl Blegen began excavations at a Mycenaean palace site in the southwest Peloponnesus and immediately discovered clay tablets in sufficient quantity to make possible, in time, the decipherment of the "Linear B" script. Taking advantage of the rich increase in our knowledge that continued excavation at Pylos (so identified by the tablets) and the decipherment of the palace archives provided, William McDonald undertook a study of the region as a whole in the Late Bronze Age (roughly 1600 to 1200 B.C.). As the project grew he realized the need for the specialized knowledge of many disciplines and from all periods of history; he gained the collaboration of a variety of scientists and scholars, sixteen of whom have contributed to the present volume. The result is a pioneering study, unique for any part of Greek history, that provides us with generous information on Messenia from the Early Bronze Age (ca. 3000 B.C.) to the present day. The authors are modest in their claims and tentative in their conclusions. Contents and methods are clearly set out, accompanied by a wealth of excellent plans and figures. A superb job of editing has produced consistency of terminology and ease of cross-reference while leaving disagreements between the contributors clear and instructive.

For the Late Bronze Age there is clear disagreement on the size of the population. McDonald and Rapp believe the agricultural potential of the land was not fully used and that the population was in the vicinity of 50,000 (approximately the same as in the early years of the Ottoman occupation in the sixteenth century and again in the middle of the second Venetian occupation in 1702). Chadwick, working from the Linear B tablets, prefers a figure between 80,000 and 100,000. There is nothing very secure in the arguments adduced for either view, but the evident degree of specialization in crafts and agriculture and the much more doubtful large merchant fleet postulated by the editors would point to a higher rather than a lower figure.

In one respect there is a significant discrepancy between the evidence of botany and that of history and archeology. H. E. Wright, Jr.'s analysis of pollen cores shows much greater cultivation of the olive in the Pylos region in the Dark Age after the fall of Mycenaean civilization than at its height when the prominence of oil is clear. Wright's effort to explain the latter evidence away is unsuccessful, and his suggestion that olive culture was more prominent in a subsistence economy than under the more developed conditions of the palace-controlled period is contrary to historical probability and the analysis of traditional practice by Aschenbrenner in this volume. Either the Carbon-14 date of 870 B.C. for the olive peak needs to be corrected to ca. 1200 B.C., as Wright concedes may be necessary, or the extent of wild olive in the maquis taking over deserted arable land in a period of severe depopulation was greater than he believes.

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H. A. HARRIS. *Sport in Greece and Rome*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1972. Pp. 288. \$11.50.

Several excellent books on Greek athletics, including Harris's own *Greek Athletes and Athletics* (1964), have appeared in recent years. Another such work would seem to "send owls to Athens." *Sport in Greece and Rome*, however, includes lesser games and exercises and extends to Roman, Christian, and Byzantine events; much new and valuable material is introduced.

The first section does lightly survey traditional Greek Olympic events and their transfer to Rome. The second section works through every kind of evidence for informal games of ball, hoop-bowling, weight-lifting, and water sports. The third section concentrates on chariot racing, tracing the nature of the sport, its racecourses, and its social and political repercussions. Roman, Christian, and Byzantine aspects are as fully developed as the Greek. An appendix presents the analyses of dreams of athletes described in the *Onirocriticon* of Artemidorus of Daldis. It is a fascinating, normally neglected view of the hopes and fears of athletes and their fellow men in the second century A.D.: "To dream that one sees one's feet burning is bad for everyone alike. It signifies the loss and



destruction of all one's property, especially children and slaves. The only people to whom this dream brings good are runners, when they see it just before competing; they will run like a cat on hot bricks" (pp. 245-46).

Harris has researched an impressive range of well- and little-known ancient literature and inscriptions and has fully exploited information derived from art. The book is strengthened by his liberal use of excellent pictures and well-translated quotations. His narrative is that of an ardent and thoroughly informed sportsman. He knows muscles, equipment, rules, and problems. He can be informative about kicking a ball with the toe, the arch, or the side of the foot, or about different bits for a racehorse. His parallels to modern sports are frequent, although—regrettably for the American reader—chiefly in English terms. His judgments on disputed questions are often independent—for example, the method of deciding the winner of the pentathlon event (pp. 34-35). The historical background is superficial. He can say, in analyzing Rome's power: "the secret lay in the character of the Roman people. . . . At some time early in the first millennium B.C. there must have been a chance combination of genes in this part of Italy which in the course of generations produced the Roman character" (pp. 47-48). But such regrettable historical vagaries never appear in his discussions of athletics.

This account of ancient sports, from childhood games to syndicate betting, makes them seem delightfully contemporary. For its liveliness, its sensible interpretations, and its thorough research, the book should prove definitive. It is a worthy addition to the valuable "Aspects of Greek and Roman Life" series.

ELEANOR G. HUZAR

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HOMER A. THOMPSON and R. E. WYCHERLEY. *The Agora of Athens: The History, Shape and Uses of an Ancient City Center*. (The Athenian Agora: Results of Excavations Conducted by the American School of Classical Studies at Athens, volume 14.) Princeton: The American School of Classical Studies at Athens, 1972. Pp. xxiii, 257, 112 plates. \$28.00.

The Agora of Athens was the center, actual and symbolic, of one of the two societies that

most profoundly shaped Western civilization and culture; the other was, of course, the Roman Forum. The Acropolis, indeed, represents for most the achievement of fifth-century Athens. Its three major buildings dominate the city visually and spiritually. The Agora had since antiquity been leveled, buried, and built over until the only visible monument stood on its western rise, a temple of Hephaistos (mistakenly called the Theseion), converted into a church of St. George. The excavations have left the Agora a still-confusing area of foundations, now planted attractively as a park. Only a few structures rise above the ground level: three of the six colossal Giants paired with Tritons that were added in the second century A.D. to the façade of a concert hall, or Odeion, built by Augustus's general Agrippa; the Church of the Holy Apostles, dating from the twelfth century, which has been left, and restored, in the southeast corner; and the two-storied portico or stoa given to Athens by Attalus II of Pergamum (159-138 B.C.), which has been re-erected along the eastern side to serve as a museum and excavation house.

Although the Agora evokes a far less visual thrill than does the Acropolis, it should stir the imagination more deeply. In it was concentrated the civic life of Athens: magistrates, council, courts, and, initially, not only popular assemblies but also theatrical productions. About 500 B.C. the assembly was moved, for reasons unknown, to a hill, the Pnyx, some distance to the southeast and plays to a theater on the south slope of the Acropolis. Despite these removals, the Agora remained central to Athenian life. Here walked and talked philosophers from Socrates to Zeno; here were held parades, races, public harangues, and other municipal functions: shops and even houses found a place around the margins down at least to the end of the fourth century.

As an instance of urban design, the Agora, like the Forum, was only gradually given form and monumentality from a haphazard layout of public buildings. Indeed this accompanied the slow loss of freedom that began during the fourth century B.C. Then, perhaps on the initiative of the great treasurer Lycurgus, a complex of two stoas enclosing a rectangular space was erected to frame the Agora to the south and to balance older and less pretentious stoas to the

north. In the third century, as noted, the Stoa of Attalus closed off the east side and balanced the row of older public buildings along the west, beneath the Hephaisteion. Under Augustus, the central area was pre-empted by Agrippa's Odeion. The real end of the history of the Agora may be marked by two events: the visit of Pausanias about A.D. 150, whose description of its then overbuilt condition is fundamental for identifying existing remains; and the sack of Athens by a barbarian tribe, the Heruli, in A.D. 267. After the sack, the remains of destroyed buildings were employed for a wall built from north to south across the eastern side and were thus preserved to assist in the modern restoration. In the fifth century A.D. the central and southern portions of the Agora were covered by an extensive edifice, probably a gymnasium and bath.

Professor Thompson was director of the excavations from 1947 to 1967. Professor Wycherley is author of studies of ancient city design and of the volume of *testimonia* in the Agora reports. From so competent a team might be expected as definitive a study as is here provided. It covers from Neolithic and Bronze Age burials to the Middle Ages. From the archeological evidence—foundations, fragments, and objects—the authors interpret both the history and use of individual buildings and the general development of city design. The concluding chapter on the earlier explorations by German and Greek archeologists and the systematic excavation by the Americans since 1931 well illustrates the progressive clarification of problems, testing of hypotheses, and identification and reconstruction, at least on paper, of buildings. The Agora excavations have been a model of meticulous digging and of careful recording of literally thousands of fragments and objects. This report brings into focus some thirty-five years of detailed work. It is the central volume of the twenty already published or planned. Its scholarship and importance are matched by a clear style, by excellent typography, and by the careful integration into the discussion of plans and of photographs of Mr. Tavlos's models, of remains, and of objects. Bibliographical references are placed in the notes. The text concludes with a concordance for the objects mentioned, an index of authors

cited, and a general index. This outstanding book is essential for the interested tourist, the archeologist, and the historian both of city design and of Athens.

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P. J. RHODES. *The Athenian Boule*. New York: Oxford University Press. 1972. Pp. xv, 351. \$25.50.

This is the only full study in English of the council that, in Aristotle's words, "handles most business of the state, along with the other officers"; another book, by Roger de Laix of Arizona, will soon appear. Rhodes's work is thorough and dispassionate, an excellent creation of Oxford scholarship that, as he says, was helped along the way by Oxford's great school of Greek historians.

Rhodes accepts the ancient opinion that Solon founded a boule that Cleisthenes replaced. I cannot agree with this, but since the putative Solonian boule plays little part in the present treatment, one need not quarrel over it. Again, Rhodes refuses to ascribe the *Athenaion Politeia* to Aristotle—an excessive caution, but a view taken by some other scholars as well. Rhodes's approach is tightly administrative rather than "social." It would be useful to know, if we can, which men served in the boule: was it really a democratic body, or was it composed of the mildly upper-class persons who serve in Congress or Parliament?

There are detailed chapters on membership, legislation, administration, and jurisdiction, this last including an appendix on the punitive powers of the boule. Most of these sections resume all the evidence with great industry and precision. Note, for example, Rhodes's careful discussion of a disputed reading in the *Anonymous Argentinensis*. The most creative aspect of his research is a study and analysis of Athenian public resolutions. Working mainly with the introductory formulas, Rhodes determines which decrees were "probouleumatic" (that is, devised or planned by the boule) and which were "non-probouleumatic" (originating elsewhere, usually in the assembly). His statistical analysis suggests a fairly even division between the two kinds of decrees in the fourth and third centuries (p. 79). This in turn leads to histori-

cal conclusions about the degree to which the assembly took the lead in democratic policy. Other scholars will wish to use the same material or perhaps vary the method.

Naturally some of the classic problems remain, even after Rhodes's judicious consideration. The work of Ephialtes was very important, but confused sources make it almost irrecoverable. Nor can we say precisely when the prytany system, dividing the administrative year into ten sections, was installed. Perhaps nobody can "solve" these puzzles, but everyone interested will study Rhodes's suggestions. The scholarly apparatus—tables and indexes—are well ordered and useful.

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PETER GREEN. *The Shadow of the Parthenon: Studies in Ancient History and Literature*. Berkeley and Los Angeles: University of California Press. 1972. Pp. 288. \$8.50.

This book is a collection of seven essays by a British journalist and novelist, now professor of classics at the University of Texas.

"The Shadow of the Parthenon" is a downgrading of Periclean Athens, as imperialist, jingoistic, financially inept, and ruled by a "quasi-dictator." Here, as Mr. Green plays the curmudgeonly role of Old Oligarch, many readers will feel a certain nostalgia for Thucydides. "Cleo Reviewed" is centrally an attack on such "Tory Romantics" as N. G. L. Hammond, Victor Ehrenberg, and Geoffrey Woodhead, with Werner Jaeger and Gilbert Murray as incidental victims. Lest it be thought that Mr. Green's approach is entirely negative, I record his approval of Milman Parry, E. R. Dodds, and Russell Meiggs. In "Athens and Jerusalem," Mr. Green tilts against theological historicism, as represented by Father D'Arcy and Professor Christopher Dawson. He concludes, "The dear city of Cecrops is not, and cannot be, the City of God." "Myths and Symbols" discusses myth as ritual, symbol, and history, with special reference to Prometheus as a socio-revolutionary symbol. Here the heroes are George Thomson, Eric Havelock, and G. S. Kirk; one of the villains is Professor Kerényi.

"The Individual Voice" is about Archilochus

and Sappho. (There is an appendix arguing for an early birth date for Archilochus, about 716/5 B.C.) Mr. Green compliments Guy Davenport's translation of Archilochus and offers some verse translations of his own, including one of a recently discovered papyrus, which, since about a third is torn away, offers much scope for conjectural emendation: he makes it a dialogue in which Archilochus' fiancée Neobule breaks their engagement. The author has written a novel about Sappho; here he translates several of her less fragmentary poems into verse and presents her admiringly as a wryly self-critical Lesbian, with an infinite capacity for self-surrender and a sharp and crystalline talent. Certain vulgarities here ("group buggery," "no kidding" [as a translation from Sappho], and "is turned on by") suggest an origin for this piece as a lecture to undergraduates. "The First Sicilian Slave War" (135–132 B.C.) sees the slave leader Eunus as modeling himself on Seleucid kings and appealing to a Messianic tradition, thus making a revolution based on two reactionary institutions, monarchy and religion. "Juvenal and His Age" reprints with some additions and deletions the introduction to Mr. Green's Penguin verse translation of the Roman satirist. The chief addition is an attack on H. A. Mason's negative answer to the question, "Is Juvenal a Classic?"; deleted are nine pages on the manuscript tradition.

Mr. Greene is an antiestablishment classicist, a phenomenon sufficiently rare to be, at moments, refreshing. He is also disarming, as when he admits in a footnote, "Greece was not *primarily* a country of economic upheavals, ecstatic irrationals, or anti-totalitarian scientific thinkers." But it is unsettling to read his claim that "it is hard to find a first-class critical study of most major ancient authors." I offer in rebuttal Dover on Aristophanes, Kirk on Homer, Otis on Vergil and Ovid, Quinn on Catullus, and Syme on Tacitus. It is a disservice to a beleaguered field to cry down this kind of serious and exciting scholarship in the name of an often dubious and almost certainly ephemeral "radical rethinking" of classical criticism.

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IOANNIS A. VARTSOS. *Athēnaikai Klērouchiai* [Athenian Cleruchies]. ("Athēna": Syngamma periodikon tēs en Athēnais Epistēmōnikēs Hetaireias. Series Diatribōn kai Meletēmātōn, 15.) Athens: M. Pechlivanides. 1972. Pp. 174. 150 D.

The Athenians, in the fifth and fourth centuries, established a number of settlements in new colonial areas and in the territory of recalcitrant allies, but many details of their procedure are obscure. A book of these modest proportions cannot solve all the problems. The author's intention is to decide how many of these settlements can be called *klerouchiai*. Herodotus uses the word *klerouchos* of the settlers at Chalcis (soon after 506 B.C.) and Thucydides of the settlers sent to Lesbos after the revolt in 428. No other fifth-century author uses the term, and the fragmentary inscriptions in which the word occurs (or has been conjecturally restored) create more problems than they solve. Evidence from the fourth century cannot properly be used to explain events of the fifth.

It is argued in this book that the settlements at Salamis (sixth century), Chalcis (506 and later in 446), Eion, Scyros, Andros, Imbros, Lemnos, Naxos, the Chersonese, Eretria, Histiaea, Brea, Samos, Aegina, Potidaea, Lesbos, Scione, and Melos were all cleruchies. The reasons given are that the settlers seem to have remained Athenian citizens, did not pay *phoros*, and were probably taken exclusively from the poorer classes in Athens, and that, in some cases, Plutarch or Diodorus, for what their evidence is worth, called them cleruchs. Did they take their families with them, and what obligations did they assume? How, for example, did their presence on an island compensate for a sharp reduction of the *phoros*, and how did five hundred or a thousand settlers take the place of an earlier population that had been displaced or enslaved, as at Histiaea, Lemnos, Aegina, and Melos? Unless these questions are taken into account, together with others which a moment's reflection will suggest, little is gained by deciding what technical name was given to a settlement—even if the evidence for such a decision were adequate.

These are some of the reasons why one cannot call this book a useful contribution to the study of Athenian policy. It could, however, have been genuinely helpful to students if it

had presented in full the original literary and epigraphic texts on which all discussion must rest, so as to fill in the gaps in Hill-Meiggs, *Sources*, and Meiggs-Lewis, *Greek Historical Inscriptions*. It is regrettable that this was not done; an Athenian publisher cannot complain that it costs much more to print ancient Greek than modern.

LIONEL PEARSON

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EDOUARD WILL. *Le monde grec et l'Orient*. Volume 1, *Le V<sup>e</sup> siècle (510–403)*. (Peuples et civilisations: Histoire générale. Volume 2.) Paris: Presses Universitaires de France. 1972. Pp. 715. 80 fr.

The series of historical studies called "Peuples et Civilisations," intended by its founders to serve as an "histoire générale," boasts volumes by such fine historians as Lefebvre, Baumont, and Renouvin. In the author of this study of the world of classical Greece they have found a peer. The series already contains a study of much the same subject by Pierre Roussel, but it was published in 1928 and is now badly out of date. That book, moreover, tried to cover the period from the Persian Wars to the Roman conquest in a single volume of about 550 pages. In arranging for a replacement the new editor acted with great wisdom. He chose France's premier historian of ancient Greece as the author and allowed him the space needed to do the job well. The present volume is the first of two, breaking at 403 B.C. and running over seven hundred pages. A second volume will carry the story to the Peace of Apamea in 188 B.C.

The book is divided into two parts, the first being a narrative history of the period. The second part, not quite half the volume, is devoted to "Civilisation," containing essays on politics, both structural and theoretical, religion, economics, and society. A splendid service is provided by the bibliographical notes that are attached to each section of the book. Though M. Will makes no claim to completeness, his grasp of the literature is unsurpassed, and each note gives a fine survey of important recent work. The plan and the apparatus of the book, therefore, are good, but the text is even better. Any writer of narrative history

must be aware of the great difficulty in giving an accurate and interesting account of events and, at the same time, making an attempt to analyze their meaning and to set them in the correct perspective. There is also a conflict between the need to tell a simple, clear, and understandable story without neglecting to give attention to conflicts in the sources and competing interpretations by scholars. M. Will has dealt with these problems in a masterful fashion, and the result is an account that is lively, interesting, careful, scholarly, thoughtful, stimulating, and a splendid introduction to the state of scholarly opinion on the history of classical Greece.

Space does not permit the citation of many instances of the book's excellent qualities, but for a brilliant example of detailed narrative combined with analysis and interpretation as well as a sense of the long-range significance of the issues, the reader may consult the section on Cleisthenes and his reforms (pp. 63-76). Here in brief compass M. Will treats an important but ill-documented, difficult, and even treacherous period with clarity and due regard for the problems and uncertainties. Political developments and constitutional technicalities are carefully examined and their interrelationship is intelligently indicated. Each new institution is examined to determine how it compares with the previous constitution, what its function might be in the political situation of the moment, and what its theoretical foundation might be. Finally the section concludes with an evaluation of the reforms from the perspectives of both past and future. M. Will shows how the program, arising as it did from the needs of a particular historical moment, nevertheless built on the lessons learned from Solon and Peisistratus. From the standpoint of the future he is able to emphasize the unique achievement of the Athenians in the time of Cleisthenes.

M. Will has written the best one-volume history of fifth-century Greece there is, graceful, learned and wise. Let us hope that an enterprising publisher will undertake a good English translation to make it available to the larger audience it deserves.

DONALD KAGAN  
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M. T. W. ARNHEIM. *The Senatorial Aristocracy in the Later Roman Empire*. New York: Oxford University Press. 1972. Pp. xiv, 246, 3 tables. \$18.00.

The purpose of this volume is to analyze the prosopographic composition of the Roman Senate during the era between Diocletian's accession and the death of Theodosius I in order that the Senate's real political and social position may possibly be discovered. As the chapters unfold, we see how "the senatorial aristocracy had considerable political power in the fourth century West, though not in the East, dating from the reign of Constantine the Great, who changed the system of appointments to high state office" (p. 4). Thus the question of the very nature of the generally accepted concept of absolute monarchy is taken under study.

Arnheim competently points out that during the Imperial period the authority of the Senate had steadily lost political influence. And, in the second half of the third century, the emperors began to advance nonsenatorials to the major magistracies without bothering first to make them senators. Diocletian even went so far as to exclude members of the Senate from the higher offices of state and to give these positions to his cronies. He was indeed an autocrat (pp. 39 ff.). But according to Arnheim's prosopographic research, Constantine reversed this trend by returning to the tradition of appointing senatorials to state office—a policy continued by his successors for as long as there was an emperor in the West (pp. 49 ff.). The author reasons that this change of imperial attitude was an attempt by the Christian rulers of the fourth century to pacify and win over an ardently pagan class of considerable landed wealth and local influence in the West. In contrast, the East never had a substantial senatorial class with which the ruler had to be concerned.

Because of Constantine's action, the emperors were forced to give the members of the Senate a greater share in the ruling responsibilities of the West, and these magistrates could use their posts as a lever to increase their wealth and political weight. In general the senatorials had two principle advantages over their non-senator colleagues. "First, they tended to be better endowed with wealth and land, and cir-



cumstances favoured the large landowner. . . . Secondly, being a close-knit intricate network of inter-related families, the aristocracy had ready-made channels for the conduction and distribution of favours and influence within their exclusive group" (pp. 170-71).

For the Roman historian, the work is an excellent example of how prosopographic information can be used in the discovery and development of a historical thesis.

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### MEDIEVAL

ALBERT C. LEIGHTON. *Transport and Communication in Early Medieval Europe, AD 500-1100*. New York: Barnes and Noble. 1972. Pp. 257. \$12.75.

It is easy enough to criticize an author for not having written a book other than the one being reviewed. It is more difficult, within the space allotted, to demonstrate that a book is not what its title claims it to be. This is the problem with Professor Leighton's *Transport and Communication in Early Medieval Europe*. Although the author states in his introduction that "the period from A.D. 500 to A.D. 1100 provides little grist for the scholar's mill," (p. 10) the title indicates that this is precisely the time span of his study. The reader will soon find that much of the evidence cited is drawn from late imperial, or earlier, sources, or from those that postdate the beginning of the twelfth century. Examples of vehicle construction are drawn from the fanciful frescoes at Pompeii, a city that was destroyed in 79 (pp. 77-78); the elder Cato (234-119 B.C.) is cited as an authority on transportation costs (pp. 157-58). At the other end of the chronological scale the curiously irrelevant information is provided that "wheat can often be shipped by rail from Kansas City to Chicago (500 miles) for about three per cent of its value" (p. 164). The extensive and very useful bibliography only confirms that many of the sources on which this study is based are noncontemporary.

One of the major disappointments of *Transport and Communications* is Professor Leighton's failure to assess more fully the efficiency of early medieval transportation. Only in his

concluding chapter has he discussed, and then all too briefly, how quickly individuals and groups of people could get from one place to another. That Julius Caesar traveled 120 miles by wagon in a single day (p. 178), or that the pony express covered the 1,975 miles between St. Joseph, Missouri, and Sacramento, California, in as little as seven days and seventeen hours (p. 184), is but remotely germane to the main subject.

There is another criticism, and this should be directed to the publisher, rather than to the author. A three-page index for a book containing much useful information is valueless. Of the names and places mentioned in this review, none are to be found in the index. This is not a very well organized study, and only an adequate index would make it valuable as a reference work for economic, social, and military historians. Commercial publishers, obviously attempting to cut costs, are by far the worst offenders in this respect.

While Professor Leighton's study falls well short of its promise, it does provide helpful details for any serious study of transportation and communication during the late imperial and early medieval periods.

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JEFFREY BURTON RUSSELL. *Witchcraft in the Middle Ages*. Ithaca: Cornell University Press. 1972. Pp. ix, 394. \$15.00.

Russell's most recent book has many of the same virtues as his earlier monograph on *Dissent and Reform in the Early Middle Ages* (1965). He marshals an impressive array of evidence to show how the concept of witchcraft developed from the time of Augustine through 1486, the year that the *Malleus Maleficarum* was published. Russell sets himself the task of commenting on every significant incident and thinker up to 1427 and then summarizes more briefly the voluminous literature of the period 1427-86. As a result of this encyclopedic urge, the chapters occasionally lose focus, but the result is probably the best general delineation of the origins of witchcraft theory now available. Russell is also careful to avoid the old rationalist liberal thesis that the early Middle Ages were skeptical and that witchcraft was "in-



vented" by Scholastics and inquisitors. Instead he shows clearly the extent to which even the *Canon Episcopi* maintained belief in witchcraft and the crucial ways in which medieval heresy contributed to ideas of witchcraft. One can say that without medieval heresy there would never have been European witchcraft apart from evil magic.

But at this point in the argument it seems that Russell strains too hard. He insists that, like heresy, witchcraft was a form of dissent and rebellion against Church and society, that such dissent is understandable in "ages of rapid social change" (which seems to include 1050–1700), and that sorcery as harmful magic must be distinguished from witchcraft as rebellion. Unfortunately it matters a good deal if the rebellion in question was merely imagined by the fearful or actually put into practice. Russell assures us that there is ample evidence that witches did exist, but his proof usually boils down to the fact that they could have existed. Because of this confusion, he often begins telling what the witches did but then shifts subtly to what their accusers said they did. Well aware of this problem, Russell throws up a smokescreen by arguing that in either case the "phenomenon" of witchcraft existed. It is this phenomenon that he describes, and in so doing he documents in detail the origin of each element of witchcraft theory: pact with the devil, sabbath, orgy, devil's mark, flight, magical salves, familiar spirits, etc. In all of these elements there is no focus except for the nebulous notion of heresy. He suggests, as Hansen did seventy years ago, that once these elements had been assembled, the witch frenzy of the sixteenth and seventeenth centuries necessarily followed. Nowhere does Russell suggest that a major reason for the popular fear of witches was not merely their association with heresy but their ability to work harm. He neglects the intimate connection between misfortune and accusations of witchcraft and goes far toward making English witchcraft trials (involving little heresy and often no devil at all) utterly incomprehensible. If Russell argues that these English trials were for sorcery and not for witchcraft, the reader should be aware that he is making a distinction not found in his sources.

Finally, Russell's concern for the various ele-

ments of witchcraft theory proves ineffective in treating the figure of the witch, the kinds of persons accused, and the dynamics of witchcraft trials. His combination of history and homily is most effective, however, in describing man's irrepressible urge to destroy dissent and apparent evil.

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G. CONSTABLE and B. SMITH, edited and translated with introduction and notes by. *Libellus de diversis ordinibus et professionibus qui sunt in ecclesia*. (Oxford Medieval Texts.) New York: Oxford University Press. 1972. Pp. xxix, 125. \$11.25.

An attentive reader may learn much from this little book about the crisis in the religious orders in northwest Europe during the mid-twelfth century, each of them acutely self-conscious, each with its own idea of its place in Christian society. A solitary surviving manuscript, perhaps the author's own, argues against wide diffusion in the Middle Ages. But since Martène printed it (rather inaccurately) in 1733, students of monasticism have noted it, and with the recent efflorescence of studies on the early Austin canons it has attracted more attention. A handy and emended text is therefore welcome. Professor Constable provides a brief, discerning introduction; Mr. Smith's translation reads well and seldom falters. The editors reasonably reject a commonly favored ascription to Reimbold of Liège (d. 1149) and judiciously renounce the hope of identifying "brother R.," the author, beyond placing him in a canonical house of northeast France or the Netherlands. The author's object is to expose the virtues of the variety of observances and customs of *servi dei* (the words occur regularly in contrast to the lay *plebs dei, christiani, fideles*), instead of proposing one way of religious life in preference to all others. His technique is to draw on Old Testament exemplars and episodes in the life of Christ to show how every sort of religious order has its place. He touches lightly on the dangers to which each sort of life is exposed, but he names none (except in rubrics). He is broad-minded, praising austerity, excusing laxity, seeing virtues in hermits, monks, and secular canons, as well as in the regular canons of whom he is evidently

one. His tone is sober. He deliberately opposes the common polemical literature of the day, where Cistercians slang Cluniacs and canons defend themselves offensively against monks.

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Cambridge*

R. IAN JACK. *Medieval Wales*. (The Sources of History: Studies in the Uses of Historical Evidence.) [London:] The Sources of History, in association with Hodder and Stoughton; Ithaca: Cornell University Press. 1972. Pp. 255. Cloth \$9.75, paper \$4.50.

The title of Professor Jack's book may be misleading to those who are not familiar with the series to which it belongs. He has not written a history of medieval Wales but rather a survey of the kinds of evidence available for such a history and the way these have been neglected or used. And he has much to say about the neglect. As he points out in the preface, if this book has any theme at all, it consists of the contrast between Welsh reluctance to write general surveys of their history and its sources and the zealotry of the English. He admirably fulfills his purpose, but it is not certain that his own survey will, as Professor Elton hopes, exhilarate readers by opening the road to unending inquiry. Part of the neglect Professor Jack describes has surely been caused by the painful inadequacy of the sources, and this may well persuade some readers that their investigation might more profitably be pursued in other fields.

On the other hand, there is no doubt that many readers will be inspired, if not by the sources, at least by the dedication and great accomplishment, which Professor Jack meticulously records, of scholars like Sir John Lloyd, Sir Goronwy Edwards, Glanmor Williams, and William Rees. Professor Jack's own survey entitles him to a distinguished place among the growing band of historians who are continuing the traditions of such scholars and building on their foundations. He has spread his net wide and writes about cartography and place names, archeology and numismatics, as well as the more obvious sources. He even finds space for the antiquaries, whose work he admires. In all these varied fields he shows a mastery of his material. He manages to lighten the burden of

such a survey by his capacity to write with lucidity and grace. In spite of his occasional negative attitude, his work will be indispensable for all future students of medieval Wales.

B. WILKINSON  
*University of Toronto*

M. M. POSTAN. *The Medieval Economy and Society: An Economic History of Britain, 1100–1500*. Berkeley and Los Angeles: University of California Press. 1972. Pp. viii, 261. \$8.75.

Professor Postan has long been known for his authoritative and specialized works on medieval and economic history, and no one has lived more intimately with the sources than he. Now he has written a survey, covering a four-hundred-year period, that brings the state of knowledge on the subject up to date. And yet it is easy reading for the nonspecialist, a work as welcome from his hands as it is surprising. The author has neglected some topics as being outside the terms of his commission; there is, for example, little tie-in with the organs of central government. The treatment of other topics is uneven, but this is because in some cases "text-book knowledge has been greatly outrun by recent researches." His chapter on "Land Use and Technology," for example, is almost twice as long as average.

Professor Postan's conclusions are not unfamiliar to students of economic history, but they are of special interest because they bear the weight of his authority. The Romans and pre-Romans, occupying the best lands, cultivated in rectangular fields using the light plow. The Anglo-Saxons added the second-rate lands of the inland plains. This heavy soil, blanketed by trees and undergrowth, called for a heavy moldboard plow with a cutting knife and iron share—a sod-buster drawn by up to eight or ten oxen. Co-aration was necessary; long and narrow plow strips were produced. Land holdings were dispersed because "the ethics of the common field demanded that every holder should have his share of good and bad land." But if some villagers had plowlands in a great field more outlying than the rest they would be at a disadvantage in making their way, to and fro, from the village crofts at the slow pace of oxen; that, too, in my opinion, would be unfair. Postan believes that lordship and the great estate were features of Anglo-Saxon so-

ciety from the start, thus coming down on the side of Trevor Aston, not Sir Frank Stenton. He defines "common field" as "a communally held and managed field even if the communal control over it was not perfect and all-embracing," thus siding with the majority. Most welcome is the authority with which the author strips off the overburden of manorialism and reveals the village community beneath. It had no legal status and it kept no records (nor needed to). "That in spite of these disabilities," says Postan, "the communal activities of the villagers can so frequently be discerned behind the silences and disguises of manorial documents goes to show that the village commune could be as active and effective a local authority as the manorial organization itself."

Postan estimates that fifty per cent of the product of an average peasant holding went for rent in cash or kind, annual or not, and tithes. Furthermore, at least half of the peasants' holdings were too small to afford sustenance throughout the year for an average family, but there was considerable part-time employment available. Yet the average servile landholder would probably have preferred more land to more freedom.

There is much up-to-date material in Postan's chapter on the wool trade and the textile industry. He believes that trade as a factor in the growth of towns has been overemphasized; freedom from the restrictive rules of feudal law and society was a major factor. The peasant class and the knightly class were both locked in place. Towns were islands of refuge in a sea of feudalism where merchants and artisans could establish a new class of society not comprehended by feudal law and society. The welcome that students will give this book will encourage Professor Postan, it is hoped, to write more on this level of communication.

W. O. AULT  
Boston University

J. R. S. PHILLIPS. *Aymer de Valence, Earl of Pembroke, 1307-1324: Baronial Politics in the Reign of Edward II*. New York: Oxford University Press, 1972. Pp. xvi, 379. \$22.00.

This book, through an examination of the career of Aymer de Valence, earl of Pembroke, presents a new and important interpretation of the troubled politics of the reign of Edward II.

The standard interpretations of the period, those of T. F. Tout and J. Conway Davies, published respectively in 1914 and 1918, both saw Edward's reign primarily in constitutional terms: as a struggle between the king and the magnates over issues of principle, a struggle to which the king responded by building up the power of his household as a kind of inner bastion, a refuge independent of baronial control then being exercised against his will by the magnates over the great public departments of state. In the midst of these struggles the earl of Pembroke, a moderate leader of consummate ability and the one attractive figure amongst a thoroughly nasty crew of otherwise disastrous politicians, in 1317-18 created and led a "Middle Party" of moderate magnates, churchmen, and administrators devoted to the principles of the famous Ordinances of 1311 yet at the same time loyal to the king.

Dr. Phillips now claims that magnate opposition to Edward II, particularly after the death of Piers Gaveston in 1312, was much less united and that personal factors and the craving of many of the magnates for access to the royal patronage played a much greater part than either Tout or Conway Davies were prepared to allow. Moreover the author claims that Pembroke, far from being a man of consummate ability, was a magnate of no more than moderate talents. Pembroke's consistent attitude, already developed in diplomatic and military service to the crown before 1307, and much affected by his close relationship with the royal family, was basically that of one loyal through difficult circumstances to a misfit of a king. Phillips also claims that the famous "Middle Party" theory is based upon inadequate and misinterpreted documentary evidence. The settlement of 1317-18 was, on the contrary, the work of a number of the more experienced members of the royal council (including Pembroke himself), the prelates of the provinces of Canterbury, and two papal envoys from Avignon. This new thesis carries conviction.

This book, however, has serious limitations. The reader will not find in it a full interpretation of Edward II's reign. Narrowly concentrated upon a series of political crises it is not a book which will make easy reading for those not already acquainted with the main outlines

and problems of the period. Thus the Ordinances of 1311, around which a good deal of the main action turns, are never summarized or analyzed. A writer can assume too much knowledge for the comfort of even erudite readers and, by so doing, obscure his own work.

J. R. LANDER

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JOHN FERGUSON. *English Diplomacy, 1422-1461*. New York: Oxford University Press. 1972. Pp. xxv, 289. \$17.75.

Mr. Ferguson has made a notable contribution to the hitherto quite inadequate study of English diplomacy during the central years of the fifteenth century. This work can fairly claim to be the only extant attempt to survey as a whole the diplomatic history of the reign of Henry VI, and therefore the diplomacy of the concluding years of the Hundred Years' War. The only other modern study of comparable importance is that by Joycelyne G. Dickinson, which was necessarily confined to its subject-title, *The Congress of Arras, 1435* (1955). The present work carries immeasurably forward not only the study of international relations but also our knowledge of the diplomats and diplomatic procedures during the period.

Any scholar who writes diplomatic history is confronted with special problems in deciding on the structure of his book. He must decide whether he is going to unravel the tangled skeins of diplomacy in the sequence in which they necessarily become tangled, that is, chronologically and more or less simultaneously. Exposition by this method is difficult, but it is the only one that presents diplomacy as it really was. The other method, which Mr. Ferguson adopts, is to treat the English diplomacy with each foreign country separately, in one chapter after another. Thus we have seven successive chapters devoted to relations with France, the Spanish kingdoms, the German princes, the Italian city-states, the Baltic, the Empire, and the papacy. This method makes possible treatment of each field of diplomatic effort in greater detail than would otherwise be the case, but it does introduce an element of artificiality into the exposition that is hard for the reader to overcome.

The eighth chapter, on "Law and Practice in Fifteenth Century Diplomacy," is exceedingly

valuable and would perhaps have served even better as a preliminary to the book as a whole than the present rather discursive introduction. The conclusion is interesting but rather too brief.

Appendix 1, spread over some forty pages, provides thirty-two most welcome lists of English envoys treating with a variety of foreign powers and of foreign envoys treating with England. Unfortunately these lists are not themselves listed, so that reference to any particular item is troublesome. Thirteen useful documents are supplied in appendix 2.

Mr. Ferguson has produced a work of fine scholarship, which becomes at once indispensable for the study of the realities of Henry VI's government. As he concludes, "impelled along a zigzag course by the various contending factions in the English Council and by the Duke of Bedford in France, English foreign policy during Henry VI's reign could only achieve the negative success of mitigating adversity."

S. B. CHRIMES

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Cardiff*

PIERRE DEMOLON. *Le village mérovingien de Brebières (VI<sup>e</sup>-VII<sup>e</sup> siècles)*. With a study of the fauna by TH. POULAIN-JOSIEN. Foreword by ERNEST WILL. (Mémoires de la Commission Départementale des Monuments Historiques du Pas-de-Calais, number 24 [1].) Arras: Archives du Pas-de-Calais. 1972. Pp. 338. 50 fr.

Unfortunately, medieval historians have not made as effective or as extensive use of archeological evidence as have their colleagues in ancient history. This failure is due in large part to the relative abundance of written evidence that survives from the Middle Ages. One might even hazard the suggestion that there is a direct statistical correlation between the survival of written evidence and the neglect of archeological data. The lower classes in medieval Europe, however, were nonliterate and our written evidence about them comes largely from a clerical elite that was tied by interest and necessity to the upper classes. Therefore it is of great importance for the medievalist who is writing social and economic history, for example, to use the results of archeological excavations so as to balance the biases evident in the written sources. Pierre Demolon's *Le village*

*Mérovingien de Brebières* is a work that provides an abundance of evidence on subjects for which extant written sources provide little or no help.

Brebières was located five kilometers to the south-southwest of Douai, very close to the modern border between Flanders and Artois. It was only four kilometers from the Merovingian royal villa of Vitry-en-Artois and was probably engaged in helping to provide food for the king and his entourage when they sojourned there. Despite this connection with a royal villa, Brebières was a very poor village and very small. At no time during the sixth and seventh centuries when Brebières existed did it have more than three dozen crude wooden huts, and we may guess that its population never exceeded one hundred and fifty men, women, and children. An exhaustive analysis of the fauna-remains by Th. Poulain-Josien suggests that the villagers had a relatively small meat component (mostly pork) in their diet. Surprisingly, their diet was not supplemented significantly by hunting.

Demolon may perhaps be criticized for not elaborating more on the historical significance of his data. As a piece of technical scholarship, however, Demolon's work is a model of clarity and accuracy. It is a new and highly worthwhile source for a period and for subjects that are poorly documented with written evidence. Works such as this should be encouraged and supported. For any nonspecialist who may harbor a notion that the medieval world was an idyllic age in human history, even a glance at the stark reality of Brebières will be a proper corrective.

BERNARD S. BACHRACH  
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Minneapolis*

EINHARD. *Vita Karoli Magni: The Life of Charlemagne*. The Latin text with a new English translation, introduction, and notes by EVELYN SCHERABON FIRCHOW and EDWIN H. ZEYDEL. Coral Gables, Fla.: University of Miami Press, 1972. Pp. 144. \$7.95.

A single adjective can describe the book, namely, disappointing. For a translation that purports to be faithful and implies that it has more scholarly pretensions than others, it falls short in both aspects. It translates "tyrannos"

as "rebels," "eorum" as "his," "sacrificii tempore" as "morning mass," "aeternae virginis" as "Immaculate Virgin," "testamenta" as "a will," and "metropolitanæ civitates" as "capital cities," to indicate only a few single and double word combinations.

In a bibliography of thirty-six books and articles, only one British, two American, and four French scholars are listed. The remainder are German, but even so there is no mention of the monumental work on Charlemagne edited by W. Braunfels. Footnotes are thin, the longest section dealing with items of clothing, but none identifying important quotations from and allusions to Suetonius's *Lives of the Caesars* on which Einhard relied.

Several debatable statements occur in the introduction. Two illustrations will suffice: the apparent conflation of Louis the Pious and Louis the German, and the remark that "in the eighth and ninth centuries marriages did not involve the Church." The best features of the publication are its pictures, maps, and genealogical table, as well as its excellent production. It is, moreover, handy to have a translation and the original Latin on opposite pages.

ALLEN CABANISS  
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R. D. BARNETT, editor. *The Sephardi Heritage: Essays on the History and Cultural Contribution of the Jews of Spain and Portugal*. Volume 1, *The Jews in Spain and Portugal before and after the Expulsion of 1492*. New York: Ktav Publishing House, 1971. Pp. viii, 640. \$15.00.

This volume contains articles by some of the leading authorities on Sephardic history and civilization: Solomon David Sassoon, "The Spiritual Heritage of the Sephardim"; Francisco Cantera y Burgos, "España medieval: Arqueología"; Cecil Roth, "Illuminated Manuscripts of Mediaeval Hebrew Spain"; Georges Vajda, "La philosophie juive en Espagne"; Jesus-María Millás Vallicrosa, "La ciencia entre los Sefardies hasta su expulsión de España"; Aharon Mirsky, "The Principles of Hebrew Poetry in Spain" (in Yevrit); Federico Pérez Castro, "España y los judíos españoles"; Nahum M. Sarna, "Hebrew and Bible Studies in Mediaeval Spain"; H. J. Zimmells, "The Contributions of the Sephardim to the *Responso* Literature till the Beginnings of the



16th Century"; Zimmells, "Codifications by the Jews of Spain"; Haim Beinart, "The *Converso* Community in 15th Century Spain"; Beinart, "The *Converso* Community in 16th and 17th Century Spain"; Israel Salvator Révah, "Les marranes portugais et l'inquisition au XVI<sup>e</sup> siècle"; William Samelson, "Romances and Songs of the Sephardim"; Ramón Menéndez Pidal, "El romancero Sefardi: su extraordinario carácter conservador"; Ovadiah Camhy, "La judeo español—facteur de conservation pendant quatre siècles"; and Henry V. Besso, "Judeo-Spanish—Its Growth and Decline." There are excellent summaries of the articles in Castilian and Yevrit, but no summaries of articles written in French.

The editor states that a comprehensive index will be included in volume 2. Without such an index the value of this work for research is reduced. One finds, for example, mention of different aspects of Ibn Gabirol and Nachmanides scattered in almost every article. Had an index been prepared for this volume, perhaps there would have been some agreement as to spelling. I realize that with four languages some inconsistencies may arise, but there should be none within a given language. Moses ben Nachman is rendered as both "Nachmanides" and "Nahmanides." There seems to have been some attempt to make most of the English and Castilian conform to a rule, but the French articles are individualistic. The editors were also unsure whether "ibn-" and "ha-" should be capitalized or whether "ben" and "Rabbi" should be spelled out or written "b." and "R." As it seems that we will be having more multilingual tomes, it might be convenient if at some time an agreement is reached on how to transliterate names and places. If this cannot be done, then the index must have cross-references so that "ibn Shaprut" or "ibn Šaprut" could be found.

As in every collection of articles, there is a wide range of quality. Sassoon's article, digested from an article he published in 1957, is so chauvinistic that it is almost useless. "Few people," Sassoon states, "seem to realize that that famous period in Europe called the Renaissance, which marked the end of the Dark Ages and ushered in the revival of Learning, was directly due to the culture built up in Moorish Spain by the Arabs and Jews in part-

nership." Really? Roth's short study on illuminated manuscripts is most useful, especially the distinction he made between Jewish attitudes toward the Bible and the Haggadah. One could only wish for color plates. Villäs Valli-crosa's article fills important gaps found in Sarton's work.

Beinart's first article on the *converso* community at the time of expulsion used much of the same data employed by Stephen Haliczzer, in "The Castilian Urban Patriciate and the Jewish Expulsions of 1480-92" (*AHR*, 78 [1973]: 35-58), but arrives at an opposite conclusion. While agreeing that the old Spanish *conversos*, who still remained Jews in tradition, could not be absorbed by the Christian community, and that there was considerable hostility between the *converso* and Christian communities, Beinart insists that it was the Crown who initiated the expulsion process as early as 1477. Beinart's position seems the more sound since he presents a picture true for both Castile and the Crowns of Aragon, while Haliczzer seems to ignore the Aragonese.

Beinart's second article and the article by Révah bring into focus the attempt of the Sephardic *conversos* to remain in their homeland. Besso examines the Ladino tradition and evaluates its chance of survival. Ladino, like Yiddish, produced a rich literature; it would be a pity if the revival of Hebrew doomed seven hundred years of creativity.

All the other articles are of an excellent caliber. It seems that the articles in this volume are confined to a "cultural" approach. I hope the second volume will consider the political and economic approach. I also hope that the second volume appears soon and with the index.

J. LEE SHNEIDMAN  
Adelphi University

C. A. CHRISTENSEN and HIERLUF NIELSEN, editors. *Diplomatarium Danicum*. Third Series, 1340-1412. Volume 7, 1364-1366. Copenhagen: Munksgaard, 1972. Pp. xx, 493. D. kr. 60.85.

By the time with which these documents are concerned Valdemar IV Atterdag of Denmark was at the height of his power. He had, by various means, not only succeeded in reuniting a chaotic country, but he had gathered all the power into his own hands and de-



prived the nobles of the influence they had become used to in years gone by. He had, moreover, taken from them much of the land from which they had derived their former glory, and there are various documents in this collection showing clearly that this process was continuing. It can scarcely be imagined that the numerous estates given to the king after his return from Avignon were merely tokens of good will.

By this time, however, these were minor matters, and there are no signs that the king's authority was questioned anywhere within his realm. With the Hansa it was a different matter, and at the beginning of this period Denmark was at war with the Hanseatic cities. There are many Danish and Hanseatic documents relating to the struggle, and it is easy to follow the process whereby peace was re-established. The various moves are clearly documented, and among the papers is the draft of the Hanseatic peace treaty. Once peace was re-established the question was how to maintain it, and the minor points of irritation are clearly shown.

Others, however, were obviously more confident in Valdemar; this is particularly noticeable in the first third of this volume where there is a lengthy correspondence between him and Pope Urban V, originating from Valdemar's visit to the pope in Avignon. The close relationship between the two is well known, and these papers show clearly how it evolved, with Valdemar on the one hand freely acknowledging the pope's authority, but on the other constantly asking him to fill ecclesiastical posts with Valdemar's own nominees—requests that were usually, but not always, granted. For his part, Urban later sought Valdemar's support for appointments he made to bishoprics—and on one occasion he refused the king's request to remove Bishop Erik of Odense to a see outside the kingdom. While the king was in Avignon there was a peculiar dichotomy between his requests to the pope, many of a purely spiritual nature, and the power politics that were taking place between Denmark and the Hansa in his absence. But perhaps the cynics are right in suggesting that the whole thing was concerned with power politics in any case.

The collection finishes with the rumblings on the Swedish succession that were ultimately

to result in the Scandinavian union under Queen Margrethe.

W. GLYN JONES

University College London

THOMAS CURTIS VAN CLEVE. *The Emperor Frederick II of Hohenstaufen: Immutator Mundi*. New York: Oxford University Press. 1972. Pp. xx, 607. \$19.25.

This book deserves full applause on every count. The reign of Emperor Frederick II covers not only a great many decades but a large number of subjects and regions. The author is equally at home in every one of them and presents a well-rounded picture in readable and intelligible style. Over forty years ago there appeared a monumental work on Frederick II by E. Kantorowicz. It was fully documented and astonished its audience as much by its learning as by the extravagance of its interpretation. Kantorowicz's scholarship made criticism difficult. Every historian felt uneasy, but nobody could quite come to terms with Kantorowicz, whose vision of Frederick owed as much to the imperial romanticism of the German poet Stephan George as to his immense erudition. K. Hampe, doyen of orthodoxy, valiantly tried to digest it in his celebrated article in the *Historische Zeitschrift* ("Das Neueste Lebensbild Kaiser Friedrichs II," 146 [1932]: 441–75). Since then a great deal has been written about Frederick II, but nobody has attempted to reopen the matter on a full scale.

Given this situation, it would have been tempting for Thomas Curtis van Cleve to fall into the trap of debunking. He could have used his own erudition to debunk either Frederick II or Kantorowicz or both. In view of the startling extravagance of Kantorowicz's picture, such a reaction would have been entirely intelligible even though it would not have advanced our understanding of Frederick II. Van Cleve has bravely resisted this temptation. His own picture of Frederick, far from being a simple reaction against Kantorowicz, is a model of judiciousness and balanced understanding. It stands independently on his own re-examination of the sources. This independence gives him the courage to portray Frederick II's staggering originality and enterprise with sympathy and sobriety. There is no attempt to fall over backward in order to disprove Kantoro-

wicz's romantic enthusiasm. The author is to be especially congratulated because the same loving care and understanding are devoted to the legal and military maneuvers as to the cultural achievements.

The one weakness of the book is the author's all too ready acceptance of a number of generalizations about the historical setting. It is not true that the "plans of the Hohenstaufen, as exemplified by Barbarossa, contemplated extending the imperial dominance over the whole of Italy and Sicily . . ." (p. 4); on page 5 the author commits himself without further ado to the view that when Barbarossa agreed to the marriage between his son Henry and Constance of Sicily, he understood and foresaw the eventual dynastic implications. But the author can be excused. Frederick II was a man of "pronounced individuality" (p. 539) and a man of "transcendent superiority" (p. 535). It is therefore legitimate to concentrate on him personally and slide somewhat cavalierly over the precise nature of the place he occupies in history.

PETER MUNZ

*Victoria University of Wellington*

ANTHONY MOLHO. *Florentine Public Finances in the Early Renaissance, 1400-1433*. (Harvard Historical Monographs, 65.) Cambridge, Mass.: Harvard University Press. 1971. Pp. ix, 234. \$10.50.

The fiscal system of the Florentine commune played a central role in both the political and the social history of the Renaissance city. The stream of money it generated enabled Florence to compete successfully with powerful neighbors, such as Milan or the papacy. But the operation of the system also created severe social and political strains within the city, which historians have not yet satisfactorily examined. Professor Molho's book is a pioneering excursion into the hitherto dimly discerned field of Florentine public finance. He concentrates his attention on the years from 1400 to 1433, during which the fiscal system underwent profound adjustments and, at the period's close, experienced a major crisis. The huge archival deposits of the Florentine office administering the public debt (the Monte) are as yet uncataloged, and Molho was unable to make use of

them. But he has made effective use of much other material, both published and unpublished—notably the deliberations of the communal councils, the records of the treasury, and private account books and memoirs. The analysis he develops is not exhaustive, but it is nonetheless original and, for present and future researchers in the same field, invaluable.

The presentation benefits from a clear organization and a vigorous style. The author considers in turn the expenditures of the commune (the costs of war far outweighed all other outlays) and the income derived from direct and indirect taxes and from forced and voluntary loans. He examines the introduction of a new system of assessment, the *catasto*, in 1427 and clearly illustrates its close ties with the older system of forced loans. The effects of high governmental expenditures both on government itself and upon the Florentine economy are also discussed. Finally, Molho is the first to illustrate the depth of the fiscal and economic crisis of 1430-33, which served as the immediate prelude to the establishment of the Medici hegemony at Florence. A suggestive conclusion indicates the points of contact, which Molho deems worthy of further study, between Florentine fiscal policy and the economic, social, and political history of the city.

Although a short study, the book succeeds in illuminating numerous aspects of Florentine fiscal history and in clearing up some points—such as the relationship between *catasto* assessments and forced loans—that have long confused historians. The comments, intended primarily as suggestions, on the relations between fiscal practice, social structure, and political history of Florence are consistently thoughtful and intelligent, and should serve as guides to future research. There is only one area in which a serious criticism can be made concerning the book. The Latin citations from manuscript sources contain numerous impossible readings. Florentine scribes rarely wrote elegantly, but almost always correctly. The mistakes are too many to be dismissed as typographical errors; they indicate that the author did not transcribe or verify his notes with adequate care.

DAVID HERLIHY

*Harvard University*

PETER CHARANIS. *Studies on the Demography of the Byzantine Empire: Collected Studies*. With a preface by SPEROS VRYONIS JR. London: Variorum Reprints. 1972. Pp. 364.

The republication, in a single volume, of twenty-two of Professor Charanis's articles and book reviews makes some of his important contributions to Byzantine history easily available. The articles assembled here consist of studies on the size and composition of the population of the Byzantine Empire from its beginning until about the thirteenth century. Professor Charanis has done some basic work on the ethnic composition of the Byzantine population. His main argument, which runs throughout these studies, is that the Byzantine Empire was, indisputably, a multinational state that, however, had two all-important connecting elements: Christianity and the Greek language and culture. While the ethnic structure of its population changed—with the large Slavic invasions, the rise in importance of the Armenian element in the ninth century and after, and with other developments—the strength of the Empire lay in incorporating these elements within its cultural traditions. Starting from this eminently sane principle, Professor Charanis can then study the hotly debated topic of the Slavic invasions (articles 2, 7, 10–13, 15–21), subject the few existing sources to a rigorous analysis, and reach valid conclusions. Accepting that there was wide Slavic settlement in Macedonia, Greece, and the western Peloponnesus, he also argues that the Slavs “eventually succumbed and became completely absorbed by the Greek race. They left behind them some Slavonic place names; but their long domination failed to affect materially Greek culture or the Greek language” (article 11, p. 258). While this particular quotation refers specifically to the western Peloponnesus, I think it is fair to say that Professor Charanis considers it applicable to the rest of Greece also (compare article 21, p. 34).

Apart from the studies dealing with the question of Slavic settlements in Greece, perhaps the most important work included in this volume is Professor Charanis's study of the Armenian element in the Byzantine Empire. In one major study (article 5) and some smaller ones, the author has brought to attention the

fact that Armenians were very active in the army, the navy, and the palace in the period of great Byzantine expansion (approximately from the middle of the ninth century to the end of the Macedonian period). Indeed, he refers to the Empire of the ninth and tenth centuries as “Graeco-Armenian” (article 22, p. 115; article 5, pp. 239–40)—that is, Greek in culture and Armenian in terms of defense. At the very least, the use of the term should serve as a reminder that the Byzantine Empire at its height was united not by race but by culture.

The first article republished here approaches the problem of population from a quantitative viewpoint. Demographic information of this kind is very limited, and one can only give approximate estimates of the population of the Empire; such estimates are really more meaningful for the sixth century than for any other period. After the seventh century, the only conclusions one can reach are qualitative and argue for a decline of the population, followed by gradual expansion from the late ninth century until the late eleventh in Asia Minor and the late twelfth in the Balkan provinces. The article does not discuss the thirteenth or fourteenth centuries; some information does exist for this period, and some demographic work is being done on it.

Written between 1946 and 1971, the articles published here all reflect a depth of scholarship and a respect for historical truth that should serve as an inspiration to the younger generation of Byzantinists.

ANGELIKI E. LAIOU  
Brandeis University

SPIRO KOSTOF. *Caves of God: The Monastic Environment of Byzantine Cappadocia*. Cambridge, Mass.: MIT Press. 1972. Pp. xviii, 296. \$18.50.

The author is an architectural historian, and his subject, the cave monasteries in Anatolia, west of Kayseri in Turkey. Christian monks are thought to have settled in this weird tufa landscape as early as the fifth century, though the churches they carved there out of the rock, and painted, date from later times, primarily from the tenth to the thirteenth centuries. The churches often imitate, in both their architecture and painting, the prevailing artistic cur-

rents of Constantinople and are a valuable source for the understanding of Byzantine provincial art.

The rock-cut churches—there are over sixty painted ones and more are being discovered every year—were first published by G. de Jerphanion between 1925 and 1942. Over the past ten years the area has been further explored by two intrepid women, N. Thierry and J. Lafontaine-Dosogne, and by Marcell Restle, whose three-volume publication of *Byzantine Wall Painting in Asia Minor* appeared in English translation in 1967. Professor Kostov's book has the advantage of being the first general survey of the Cappadocian churches in English in one volume. It is divided into three sections: "The Setting," "The Buildings," and "The Paintings," followed by a catalog of the major churches (sixty-six entries) arranged in chronological order.

The book has a number of shortcomings. It suffers from flaws in organization, from inadequate photographs, and from a regrettable unevenness in the editing, whereby whole sections are left in extremely unidiomatic English (especially pp. 114–41). Kostov opts for a chronological, rather than topographical, arrangement within his two main divisions of architecture and painting, but then confuses the reader by, for example, placing his chapter on the "Archaic Phase" of architecture (A.D. 850–950) before his chapter on the "Iconoclastic Phase," though the period of iconoclasm ended in 843. He frequently introduces discussions that properly belong in the painting section into his architectural section, and inserts essays on the fundamentals of Byzantine liturgy and iconography somewhat at random throughout the text; for the interested layman, these are hard to locate and, for the specialist, only serve to disrupt the train of the more technical arguments.

Kostov includes ground plans for most of the churches, but his photographs are of poor quality. Many are out of focus, and details are often only blow-ups of more general views; in fact, the only satisfactory pictures of the frescoes, at least, are those reproduced from Restle's volumes. Kostov's arguments for the dating of some churches, as well as for the sources of certain architectural forms, might have been

strengthened had they been accompanied by illustrations of comparative material.

Nevertheless, this book has valuable qualities. Kostov attempts to place these monasteries in their geographical and cultural context, not limiting his discussion to questions of iconography or style alone. He pays close attention to the architecture of these churches at a time when their paintings are attracting the most interest. He analyzes the various pictorial programs with particular emphasis on the choice of scenes and the relation of the cycles to their architectural framework. In the catalog Kostov gives us not only the modern Turkish name for each church, but also its other names and the earlier spellings found in older reports. The volume concludes with an annotated bibliography of the most important writings on the subject, useful for the beginner and the specialist alike.

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ROBERTO DI CLARI. *La conquista de Costantinopoli (1198–1216)*. Critical study, translation, and notes by ANNA MARIA NADA PATRONE. (Collana storica di fonti e studi, 13.) Genoa: Università di Genova, Istituto di Paleografia e Storia medievale. 1972. Pp. xiii, 294. L. 5,200.

This is the first Italian translation of the chronicle of Robert of Clari, a prime Western source for the Fourth Crusade. The value of Robert's chronicle lies in its occasional differences from other Western accounts and especially in that the author reflects, unlike the semiofficial propagandistic "line" of Villehardouin, the personal views of a modest, obscure, Picard knight. No Western chronicler describes in such detail the marvels of Constantinople: its magnificent palaces, gates, columns, and especially churches and chapels with their innumerable precious relics of earliest Christianity (including two pieces of the True Cross, "large as a man's leg and three feet wide"). Patrone exhaustively annotates every passage, comparing each with other readings or interpretations of Villehardouin, Gunther of Paris, Anthony of Novgorod (especially regarding relics), and the Greeks, Nicetas and Mesarites. Though reaching no startling new conclusions, she makes useful observations, which are, for the most part, convincing: that Robert went home in

the spring of 1205, not in 1206; that Robert composed his work not so much to identify and place in context for the monks of Corbie the relics he brought back (no less than fifty-four—one wonders what each of the great lords brought!), nor at anyone's specific request (for example, that of the abbot of Corbie), but simply, in the manner of the age, to describe for his circle the expedition and what role he had played in these "heroic" events. Contrary to others, Patrone believes that, though illiterate, Robert had a remarkable memory for facts and events (despite occasionally confused chronology) that was supplemented by the recollections of his comrades. Despite the frequent dearth of solid evidence (we do not even know Robert's age), Patrone succeeds, through deduction or the gleaning of evidence based on records of the life of his class in Picardy, in giving us in six chapters some picture of Robert's character and culture and in placing him in the context of his time.

She emphasizes Robert's censure of the leaders of the crusade for cupidity and dishonesty regarding division of the enormous booty captured and stresses that the Latin prelates, between the assaults, turned the expedition into a holy war against the Greeks by branding them "traitors and assassins . . . and worse than the Jews" and then giving absolution to all Latin participants. Previous editions of the original text (P. Riant, Charles Hopf, and especially P. Lauer, on whose edition she bases her translation) are discussed along with French, German, and English translations. This Italian translation has certain nuances lacking in E. McNeal's English translation (1936), but the latter is still quite adequate for those with no Italian. The copious references to the most recent studies (A. Carile, C. Brand, H. Roscher, etc.) and the critical apparatus, extensive annotations, and analytical indexes of places, persons, and sources make this a very useful reference work for all scholars of the crusades and especially of relics seized in Constantinople.

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BENJAMIN HENDRICKX. *Hoi politikoi kai stratiotikoi thesmoi tēs latinikēs autokratōrias tēs Kōnstantinoupolēōs kata tous prōtous chronous tēs huparxeōstēs* [The Political and Military

Legal Foundations of the Latin Empire of Constantinople during the Early Years of Its Existence]. Thessalonica: [the author]. 1970. Pp. 190.

The Fourth Crusade was a decisive factor in the historical evolution of the Balkan peninsula and western Asia Minor. But the Latin Empire, one of its many results, was a transitory phenomenon whose political influence became more and more insignificant until finally the Empire itself ceased to exist. By its very existence, however, it was a force of some importance and by its institutional development, a body politic by no means unattractive for study. Hendrickx's book, a doctoral dissertation submitted to the faculty of philosophy of the University of Thessalonike and devoted to an analysis of this institutional development, is not, therefore, without general interest.

Hendrickx makes the analysis of three documents the point of departure of this study: the agreement of March 1204, the *Partitio Romaniae*, and the agreement of October 1205. The first laid the basis for the feudal organization of the Empire and defined the position of the Venetians in it; the second provided for the actual partition among the crusaders, the emperor, and the Venetians of the lands conquered or expected to be conquered; the third, while confirming the agreement of 1204, defined more strictly some of its provisions. These three documents comprised, according to Hendrickx, the constitutional basis of the Latin Empire and served as the sources for the development of its institutions. These institutions, both political and military, were essentially feudal in nature and, as a consequence, Western in origin. Only in the ceremony of imperial coronation does the author see any Byzantine influences.

Greek apparently is not Hendrickx's native tongue. In general, however, despite some rough spots here and there, his narrative is very clear and his scholarship sound. These qualities make his book a useful contribution to scholarship. This usefulness is increased somewhat by the French summary of its contents appended by the author.

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ANGELIKI E. LAIOU. *Constantinople and the Latins: The Foreign Policy of Andronicus II, 1282-1328*. (Harvard Historical Studies, volume 88.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xii, 390. \$18.00.

This detailed account of the forty-six year reign of the Emperor Andronicus II is a welcome addition to the literature on the Paleologan era (1261-1453), a period that until recently has received less attention than the earlier centuries of Byzantine history. The author's emphasis is on the relations of Andronicus with Western Europe, especially with Venice and Genoa, which held important commercial privileges in Byzantium; with the Angevins and members of the French royal house who dreamed of conquering the Empire; and with the papacy which desired a united Christian church. Dr. Laiou rightly stresses, however, the influence of domestic issues on foreign policy and thus also includes a substantial discussion of internal conditions in the dwindling Byzantine Empire.

For twenty years after Andronicus ascended the throne in 1282, Byzantium's Western adversaries were occupied with the war of the Sicilian Vespers. Thus the new emperor felt he could safely withdraw from involvement with Europe, and made two significant changes in foreign policy: he repudiated the Union of Lyons negotiated by his father Michael VIII in 1274, and concentrated on the neglected defense of Asia Minor against the advancing Turks. His policy failed, however; by 1302 the Ottomans and other Turkish emirates were well established in western Anatolia. During the critical period 1302-11, Catalan mercenaries, hired to fight the Turks, turned against the Byzantines and ravaged Thrace and Macedonia. At the same time, Charles of Valois, claimant to the Latin Empire of Constantinople, was able to acquire an impressive number of allies for an expedition against Byzantium, which fortunately never materialized. Dr. Laiou clearly demonstrates Andronicus's dramatic shift in policy after 1311, as he not only sought a rapprochement with the West, especially the Ghibelline states, but also initiated negotiations with the papacy on the union of the churches. But it was too late; his earlier isolationist policy had determined the fate of Byzantium, which would remain a small

Orthodox Balkan state until its final collapse in 1453.

The author presents evidence that Andronicus had been willing to discuss union as early as 1311, and in the years 1324-27 seriously considered returning to the religious policy of his father. The title of the book might lead one to expect more discussion of Andronicus's relations with the unionist party in Byzantium, and with Catholic missionaries such as the Dominican Simon of Constantinople, who had correspondence with the imperial court. This is a minor blemish, however, in a carefully organized monograph of meticulous documentation, which should prove invaluable to scholars of this period. The author has made use of an impressive array of Western and Greek sources, and has included a useful bibliographical essay at the end of the book.

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RICHARD F. KREUTEL, translated, edited, and with an introduction by. *Leben und Taten der türkischen Kaiser: Die anonyme vulgärgriechische Chronik Codex Barberinianus Graecus 111 (Anonymus Zoras)*. (Osmanische Geschichtsschreiber, number 6.) Graz: Verlag Styria. 1971. Pp. 277. 140 Sch.

The defective manuscript Vaticanus Barberinianus Graecus 111 preserves the middle years, from about 1373 to 1513, and thus only the torso, of a demotic Greek chronicle of the Ottomans. Sometime in the seventeenth century a Christian author composed this work (called the Anonymous Zoras after its editor) following earlier compilations, mostly Italian. The historical merit of the Anonymous Zoras rests in its exposure of the mind of one observer hostile to the Ottoman enterprise. Dr. Richard Kreutel, known and respected for his exemplary translations from the Turkish, here publishes an excellent rendering of the text, adorned with helpful notes and glossaries. He suggests that the author, possibly a Venetian subject, was not a native speaker of Greek but a polyglot European, "ein kleiner Dolmetscher." While further investigation of dialects is necessary to decide whether this source is so conspicuously idiosyncratic, the few slips adduced (if they are slips and not current usage) are not persuasive.

The Anonymous is a peculiar and frustrat-



ing author, fortunate to have found both a sympathetic editor and a distinguished translator. Familiar with Greek, Italian, and Turkish, his unprobing intellect did not seek out Ottoman sources of information on the early period but, at best, interpolated Turkish expressions into the texts of his prototypes. A separate folio originally belonging to the manuscript relates events of 1596; so, even before the Anonymous set to work, Europe could have read printed translations of an early Neshri as well as two recensions of the Anonymous Giese. In his notes Dr. Kreutel confronts the Anonymous with his own translation of the Giese edition of Ashiqpashazade, a source for Neshri, often obscure certainly but always informative. The Anonymous either knew of no such sources or rejected them. It is provocative to realize that a European armchair historian of the time was better equipped to prepare an Ottoman history than at least one literary levanter. Dr. Kreutel forces us to ask why.

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#### MODERN EUROPE

SALO WITTMAYER BARON. *A Social and Religious History of the Jews: Late Middle Ages and Era of European Expansion, 1200-1650*. Volume 13, *Inquisition, Renaissance, and Reformation*; volume 14, *Catholic Restoration and Wars of Religion*. 2d rev. ed.; New York: Columbia University Press; Philadelphia: Jewish Publication Society of America. 1969. Pp. vi. 463; 412. \$10.00 each.

Professor Salo Baron's monumental, comprehensive social and religious history of the Jews has now reached the mid-seventeenth century. As Baron pointed out when he began publishing this revised and enlarged version back in 1952, the recent, momentous developments in Jewish history—the holocaust, the greatest tragedy in the long career of the Jews, and the establishment of a viable Jewish state in Palestine for the first time since the fall of the temple—have put a new perspective on the whole historic career of the Jewish people (vol. 1, p. vii). Part of this new perspective has been a reconsideration of how the Jews entered the modern world, how they interrelated with the European societies in which they resided, and

how this generated the forces that led to the catastrophic end of Jewish life in the Hitlerian world and to a form of the fulfillment of the millennial hope of the rebirth of a Jewish nation on the ancient soil of the Jews.

With this new focus, plus the immense accretions of fundamental information about ancient, medieval, and modern Jewish history, especially in the last three decades, as many academically trained researchers in several historical fields have turned their interests to this area, Professor Baron has been putting together a grand synthetic picture of the career of the Jewish people from its ancient origins to modern times. Utilizing an amazing amount of standard and recent scholarship, Baron has tried to delineate what he sees as major lines of development leading to the twentieth-century scene. (About half of each of the volumes being reviewed here are footnotes, encompassing an extraordinary amount of the primary and secondary literature available in most European languages and in Hebrew, Yiddish, and Ladino. Even more impressive is that this assemblage of sources has been accomplished by Baron and his wife alone, without any coworkers or research assistants [see volume 13, page vi].)

In view of the vast range of events in Jewish history, taking place all over the European, Mediterranean, and, later, American worlds, Baron has outlined in the preface to volume 9 his schema to deal with this in the period of the late Middle Ages and the era of European expansion, 1200-1650. For Baron, it is at approximately 1200 that "the fateful shift of the center of gravity of Jewish life and thought from Muslim to Christian lands" occurs (vol. 9, p. v). And, he sees the Treaty of Westphalia and the effects of the Thirty Years' War as the first bases for Jewish emancipation and for Jewish entry into the modern, capitalistic, Western European world. Volumes 9 through 12 have dealt with the effects of the medieval Christian world on the Jews. Volumes 13 and 14 cover the transition to the modern world, in terms of the effects of the Spanish and Portuguese Inquisitions, the Renaissance, the Reformation, the Catholic Restoration, and the Wars of Religion on the Jewish communities, principally in Iberia, Italy, the Holy Roman Empire, and the German states. The primary

emphasis in these two volumes is on the social, political, and religious developments in the Christian world that affect the Jewish communities and on the social more than the religious changes going on within the Jewish communities. The Jewish religious developments, especially the messianic movements culminating in Sabbatai Zevi's proclamation of himself as the Messiah in 1666 and in the rise of modern Hasidism, are given slighter treatment, as are those developments presaging the emergence of reformed or modernized Judaism. Presumably the subsequent volume covering the period of Sabbatai Zevi will go into the details of the long build-up to this traumatic event in Jewish history. And, one also presumes, other factors relevant to major modern religious developments will also be treated later on. The topographical division of the material enables Baron to concentrate on certain aspects at one time in the geographical context of specific European Christian political entities. It has, however, the defect that developments in the Jewish world taking place outside these geographical and political limits are not adequately explained or sometimes taken into account.

The first half of volume 13 deals with the crisis of Iberian Jewry engendered by the forced conversions starting in 1391, by the establishment of the Spanish Inquisition to police the *conversos*, by the expulsion of the unconverted Jews from Spain in 1492, and by the forcible conversion of the Jews in Portugal in 1497. Baron carefully traces the developments that led to the onslaught against the Spanish and Portuguese Jewish communities, ending the Golden Age of Iberian Jewry and creating the Marrano diaspora. He then traces the New Christians and/or Marranos as they fled all over Europe and to the Spanish and Portuguese overseas empires.

The tragic history of the Marranos and the economic, political, and cultural effects this drama had in Iberia and elsewhere are now a subject of major investigation by many scholars working from many different perspectives. In the few years since Baron wrote his volume much new material and many new proposed interpretations have come out, as well as a good deal of controversial literature. I think most scholars in the field would still agree with

Baron's summation that "unwittingly the Inquisition, too, set in motion historic trends which, in the long run, greatly fructified the development of modern civilization. Not the least among these new factors was the rise of a vast and variegated Marrano diaspora, which, like other great migrations of religious persecutees, served as a yeast in the growth of new forms of life and thought" (vol. 13, p. 63).

But many of us working on the effects of the Marrano diaspora would feel that Baron tends to minimize what happened. The substantial new contributions of Carlos Noreña on Luis Vives, of Claudio Guillen on Las Casas, of Stephen Gilman on the author of the *Celestina*, of Karl Kottman on Luis de Leon, of Martin Cohen on the Jews of Mexico, of myself on Isaac La Peyrère, as well as the controversial works of A. J. Saraiva, I. S. Revah, Elias Rivkin, and many others have both increased our basic information about what happened and what this may have led to. Baron usually takes a less controversial stand than others on such matters as whether Columbus was of Jewish origin. Baron tends to see the New Christians, such as Vives, mostly in terms of their Christian apologetics rather than in terms of the heterodox views that may have been engendered by their situations in the Iberian world and their roles in undermining the standard Christian outlook. The Kabbalists who became Christians are mainly written off once they leave the fold. Figures like Leone Hebreo, the son of Don Isaac Abarbenel, are truncated to just their general influence on the Christian world, with their Jewish contributions ignored or minimized. The mix that probably existed of Jews, Marranos, and New Christians—each advocating views that reflect the kind of new amalgamation taking place by the immersion of Jewish scholars in Christian topics—and of Marranos and New Christians from Servetus to Valdes, Vives, Montaigne, Sanches, Ricci, and a host of others is not given much consideration as a major force in the emergence of new ideas. Similarly the Marranos and New Christians who became involved in the Reformation and Counter Reformation are given short treatment if they remained Christians after leaving Iberia. Baron concludes his discussion of the Marranos by saying that "the large and highly

diversified Marrano diaspora thus greatly influenced much of Western, as well as Jewish history. Its impact on the nascent capitalist evolution is yet to be told in detail. . . . Because the exodus of the Marranos from Portugal and, to a lesser extent, from Spain lasted much longer than the exodus of the Huguenots after the revocation of the Edict of Nantes in 1685, it kept the Jewish communities and their neighbors in a state of more-or-less permanent fermentation for two or three centuries. Not the least were its seminal effects on the cultural life of the Jews, and to some extent, of the Christian world, which was then in the throes of the humanistic Renaissance, the Protestant Reformation, and the Catholic Restoration" (vol. 13, p. 158).

Some of us suspect, or would claim, that the impact on the Christian world of the sixteenth and seventeenth centuries was much greater and led to many basic modern intellectual tendencies including modern skepticism, Bible criticism, and secularism. Baron sees the roles of the Jews and Marranos in the Renaissance, the Reformation, and the Counter Reformation as suppliers of data, such as Hebrew scholarship, and as middlemen of ideas. The New Christian humanists are seen as having become part and parcel of Christendom. The evidence suggests to me that all of the groups—Jews, Marranos (whom I see as both a separate group that was part Jewish and part Christian, perhaps, in many cases, being what Yerushalmi has called "potential Jews"), and New Christians (whom I see as Christians with a Jewish emphasis)—played roles in generating new forms of Judaism and Christianity for the modern world. Baron, on the other hand, considers mainly the baleful effects the Reformation and Counter Reformation had on Jewish life.

Volume 14 is devoted first to the Counter Reformation and how it brought back restrictions on the Jewish communities and persecuted the Marranos returning to Judaism, principally in Italy. The second half of the volume deals in great detail with what happened town by town in the Holy Roman Empire during the Thirty Years' War. Only brief attention is given to the influence of outside Jewish forces, in the tolerated Jewish communities of Poland, Turkey, and Holland, on what was happening in the Empire. The net effect of the Thirty

Years' War Baron sees as a major turning point in Jewish history. First, since the war was fought over Catholic-Protestant issues, the Jews were no longer the most persecuted religious minority. Second, Jewish bankers, merchants, and contractors could play a vital role in providing the resources for the statesmen and generals, and thus became important in the emerging new order. Third, the military and political deadlock led to an "enforced mutual toleration of Catholics and Protestants" that was "bound deeply to affect the relations between the Jews and the majority peoples" (vol. 14, p. 294).

The latter claim, taken by itself and taken with the broader thesis that the Treaty of Westphalia was the beginning of Jewish emancipation, is bound to raise many questions and doubts. Baron ends volume 14 by coupling what he sees as the results of the Thirty Years' War with what he will presumably be dealing with in the next volume or two, the Jewish resettlement in Holland, England, and the New World. All together these developments may provide a base for his thesis. To this point it seems to me that the developments in Holland and the Dutch Empire play a crucial role in exhibiting how a tolerated and legally existing Jewish (rather than Marrano) community could function as an integral and vital part of a modern commercial state. The ideological debates in the French, English, and American Enlightenments, as well as the Judeo-centered messianic theories of the English and American Puritans, of Isaac La Peyrère, of Huguenot theologians like Pierre Jurieu, and of Jansenist millenarians like the Abbé Grégoire, provided an intellectual basis for the acceptance of Jews as equal participants in either modern secular states or messianic states. These views were not generated out of the deadlock in the Thirty Years' War but rather out of philosophical and theological considerations that were in the forefront in societies where the Jews were a tiny aspect of the social and political scene, namely England, France, and the English colonies in America.

To conclude, Baron's volumes are a most impressive compendium of what is known about Jewish life in the period 1400–1650, within the topographical domains that he treats. He has synthesized an amazing amount of material

and supplied a base for research on almost everything he touches, with the thorough bibliography in his notes. His *History* is, and probably will remain for decades to come, the most complete effort available. It already needs supplementation on the basis of new studies, but this was bound to be the case since fundamental research is going on all of the time on various aspects of Jewish history. The work is well written and full of stimulating interpretations that scholars will be debating for years to come. These volumes, plus the preceding ones and presumably the ones to follow, are indispensable for work in Jewish history. One can only be grateful to Salo Baron for his lifelong dedication to Jewish history and his desire to present the material in terms of a mid-twentieth-century focus.

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J. R. HALE. *Renaissance Europe: Individual and Society, 1480-1520*. (History of Europe.) New York: Harper and Row. 1971. Pp. 350. \$8.95.

Professor Hale, while "not ignoring the events on which a sense of chronology depends," wishes especially to understand "the quality of the lives" and "to suggest what it was like to have lived then." He is aware that an exploration of "states of mind" in what he calls "majority history" is hazardous because it can only be based on inference from "material circumstances" and accounts by individuals of how they thought and felt. In trying to cover all of Europe from 1480 to 1520, the author is faced with several problems—namely, how to deal with the enormous regional diversity; the lack of a genuine European community except of scholars, diplomats, churchmen, and possibly merchants (clearly minorities); and the extension of many deeply held attitudes far back or far forward in time. Indeed he is hard put to keep within his time limits and is compelled to stress the geographical variations.

Moreover, as he declares, "decisions about . . . what areas of experience to explore are ominously subjective." Professor Hale's historical commitment is toward "then-mindedness," and as in all such approaches the crucial question is whether it is "our" "then-mindedness" or "theirs." In his book, as perhaps in all such at-

tempts, it has to be "ours," for we supply the categories. The author meets the problem by offering a fairly conventional list of chapter headings reflecting the areas into which historians place themselves: "Political Europe," "Individual and Community," "Economic Europe," "Class," "Religion," "The Arts and their Audience," and "Secular Learning." But with a bow to structuralism he prefaces these with his first chapter on "Time and Space" in what is perhaps the most original treatment within his book. Each succeeding chapter is strongly original in its emphases, showing over and over again that the people living then did not think in terms of the categories we seek to put upon them. He offers many first-rate insights along the lines of how "they" put things together or broke them down. Despite his submission to the requirements of a semitextbook or handbook, it is clear that he has really written neither of these but something that goes a long way toward the reconstruction of the consciousness of that time and area.

Of what use then is such a book? An undergraduate, even an exceptionally intelligent and knowledgeable one, would find it extremely difficult to follow in many places because it is often presented in terms of scholarly discussions of the problems. A history graduate student, particularly one wishing to work in these areas, would find Hale's efforts to "put it all together" extremely useful; his cautions and judgments of the proper emphases to be given various topics are salutary. Scholars, too, would find these judgments valuable, though, despite his rather remarkable garnering of evidences and *exempla*, they will not find many of the topics considered new to them (but some they will). He does make mistakes, for instance lumping Pomponazzi together with the humanists and Pontano with the philosophers, but these are few and not essential. And in the area of my own concerns, his extensive treatment of humanism seemed sound, reliable, and congenial. All in all, then, this is a fine book, though it is hard to place it—neither original research, nor *la grande synthèse*, nor textbook or handbook, nor bibliographical survey and evaluation; and yet something of all of these.

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HENRY KAMEN. *The Iron Century: Social Change in Europe, 1550-1660*. (History of Civilisation.) New York: Praeger Publishers. 1971. Pp. xiv, 464. \$15.00.

Henry Kamen's *Iron Century* treats of a great variety of topics: perceptions of space and of time, population structures, rise of prices, decline in real wages, increased value of land, the role of refugees in the expanding capitalist enterprise, the mode of life of the nobility, the bourgeoisie and the peasantry, poverty and poor relief, banditism, slavery, spiritual revival of the Counter Reformation, literacy, universities, religious toleration, witchcraft, peasant and urban revolts, and the revolutions of the 1640s. On all these subjects, and on many more, the reader will find a wealth of valuable information. The material is derived from a careful and critical reading of the recent social historians and from the author's own research in contemporary sources, from which he gives many an apt quotation.

The book is conceived as an "essay in quantitative history." Indeed, the influence of Braudel and of the *Annales* school is strongly felt. The author, however, believes that "art, culture and science" are so important that they deserve a separate volume; thus their virtual absence from this book is neither accidental nor a result of blindness to these sides of the story of man. Nevertheless, a penetrating account of the theology and the mood of the Counter Reformation (chapter 7) fills in at least a part of this lacuna.

The many examples on which Dr. Kamen bases his findings cover the whole area from Portugal to Russia and from Sweden to Sicily. Thus Europe "from the Atlantic to the Urals" emerges as a palpable reality: it forms one society whose parts seem to be connected with each other, and which is beset by the same types of problems, though the solutions to these problems may differ from place to place. Inevitably, Dr. Kamen has to deal with the question of the "general crisis of the seventeenth century." In fact, each of the first six chapters, devoted to the economy and social structures, leads up to this problem; in chapters 9, 10, and 12 the author comes to grips with it directly, while giving a succinct and incisive criticism of the whole crisis theory (pp. 307-09), at least of its more simplistic forms.

"There are," he says, "two, distinct senses in which the concept of a 'General Crisis' can be manageably discussed. The first of these . . . is the notable recession in the European economy, observable in the decade of 1610-20 and pronounced after about 1640. The second of these is the series of governmental crises of the decade 1640-50" (p. 309).

The socio-economic recession, which the author discusses in detail, was complex in character and hit all of Europe. The lands in the vicinity of the Mediterranean basin, the most advanced part of Europe, never recovered from it. Northern Europe eventually effected a recovery, though at a high cost in human terms. Amidst the generally unsettled conditions, land and various other forms of rent, as well as government offices (especially in France), became the most desirable form of investment. As a result of this, the pressure on the lower segments of society increased. In Western Europe the townsmen captured the countryside and depressed much of the populace to the status of a rural proletariat, while in Eastern Europe, where there was enough land, but labor was short, the landowners both captured the cities and imposed "new serfdom" on most of the peasantry. This is the background to the peasant and urban uprisings that are so ubiquitous in the sixteenth and seventeenth centuries. When the administrative and fiscal needs of the governments, occasioned by the pan-European war, rose to unprecedented heights in the 1630s, this burden produced a series of explosions all over Europe that led to the general governmental crises of the 1640s. These revolutions, taking different courses in different parts of Europe, were all stillborn, and the general pacification that followed them was characterized by a conservative reaction: the aristocracy became socially more firmly entrenched than ever, and absolutism emerged as the new form of government.

Obviously one cannot do full justice to Dr. Kamen's complex work in a brief review. A few questions and criticisms are, however, in order. The term "bourgeois" as used by the author covers such a variety of groups that it seems to include almost all the "forward-looking" elements of society. A person like Pascal would be surprised, and a president of the Parlement of



Paris indignant, if he saw himself qualified as a "bourgeois." Dr. Kamen regards the yearning of the upper bourgeoisie to acquire land and "live nobly" as a "backward step" (p. 178). If one adopts the system of values of a chamber of commerce, this may have been so. But the upper bourgeoisie who bought land and offices and then penetrated into the ranks of the nobility not only helped to rejuvenate the noble class, but also raised its educational standards, thereby helping to produce that "polite society" without which the cultural picture would have been bleak.

The popular uprisings, which Dr. Kamen so ably dissects, did not begin in mid-sixteenth century: they have a long pedigree, going back to the fourteenth century, and in some places even earlier. Would we then be justified in extending the "crisis" period to cover four or five centuries? This would indeed be a *reductio ad absurdum*, and Dr. Kamen is of course far from any such notions. Moreover the uprisings did not end in mid-seventeenth century, but went right on throughout the age of Louis XIV, though with less intensity than in the 1640s. Perhaps conservatism and absolutism were not as all-pervasive and successful as the author leads us to believe. Furthermore, in examining political thought we find that many of the absolutist writers of the early seventeenth century had put forth claims that went much farther than those advanced by someone like Bossuet; in this sense, at least, we can properly speak of a retreat of absolutist theory in the seventeenth century. Absolutism, no doubt, existed in the minds of Louis XIV in the first decades of his personal rule, of Colbert, of Peter the Great, or of Charles XII. But it was never fully implemented until the French Revolution; nor did it ever last more than a few decades. In France, for instance, a noticeable retreat from it began in the 1690s. In each instance it was the aristocracy, whom Dr. Kamen tends to regard as its ally, that helped to set absolutism back. I must add that none of these critical considerations detract from the very real value of this thought-provoking book.

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YOSEF HAYIM YERUSHALMI. *From Spanish Court to Italian Ghetto. Isaac Cardoso: A Study in Seventeenth-Century Marranism and Jewish Apologetics.* (Columbia University Studies in Jewish History, Culture, and Institutions, number 1. Edited under the auspices of the Center for Israel and Jewish Studies, Columbia University.) New York: Columbia University Press. 1971. Pp. xx, 524. \$20.00.

The scion of a Marrano family, Ferdinand Cardoso was born in Portugal in 1604 but was raised and educated in Spain, where he lived for several decades as a nominal Christian. As a teacher of philosophy and medicine at his alma mater, the University of Valladolid, and then as a practicing physician in Madrid, Cardoso participated fully in the rich intellectual life of Spain. In 1648, when he was forty-four and at the height of his career and in no apparent danger from the Inquisition—he had testified before this body in 1634 on behalf of another New Christian, his only recorded contact with the Holy Office—Dr. Cardoso moved to Venice and five years later to Verona, where he died in 1683.

Fernando and his brother accepted Judaism, becoming Isaac and Abraham respectively. In spite of his duties as a physician to the Jewish community of Verona, Isaac Cardoso was able to publish in 1673 "a sumptuous Latin tome of over 750 folio pages, entitled *Philosophia Libera* . . . , the summary of a lifetime of thought in science, medicine, philosophy and theology" (pp. 216, 218). Six years later his *Las Excelencia de Los Hebreos* appeared; Yerushalmi calls it "his apologia, and it is so in a double sense: a vindication of Judaism and Jewry before the nations, and a culminating justification of his own life and the choice he had made" (p. 350).

Like M. J. Bernadete's *Hispanic Culture and Character of the Sephardim* (1952), this work by Yerushalmi views the Sephardic world from within. The seventeenth-century Marrano who fled the Iberian peninsula and returned to Judaism was the descendant of Jews who had become Christians in Spain before 1492 or who had been forcibly converted in Portugal in 1497 without the possibility of flight. What must have been the character and spirit of these men and women, like the Cardosos, to maintain and even to remember their Jewish identity? They were forever haunted by a



Spanish Inquisition that persecuted the *conversos* and their descendants by fire and torture for not being good Christians. Did the Inquisition, by these methods, hope to bring about total assimilation? And, at the same time, the Spanish laws of racial purity—*limpieza de sangre*—which had so patently violated the teachings of the Church, prevented this same assimilation of these New Christians. Perhaps this impossible dilemma could only be resolved by martyrdom or flight and return to Judaism.

Yerushalmi's book opens up to the student of early modern Europe, who is well acquainted with Spinoza's contributions, an aspect of Marrano history hitherto almost unknown.

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J. M. ROBERTS. *The Mythology of the Secret Societies*. New York: Charles Scribner's Sons. 1972. Pp. x, 370. \$17.50.

The secret societies with whose mythology this book is concerned are those held responsible for the French Revolution and for later attempts to subvert regimes under Napoleon and Metternich. Mr. Roberts has set out to trace the growth of a belief in international revolutionary conspiracy that seemed compelling to nineteenth-century statesmen down through Disraeli's day. His stated aim is to "explore the subject as a whole." He deliberately leaves limits undefined and devotes as much attention to the societies as to the myths they inspired. France and Italy furnish the main scenes of action; Abbé Barruel and Buonarroti loom large among mythmakers; the Masons, the Illuminati, the Carbonari are the chief societies discussed.

The book is loosely organized along chronological lines encompassing a century between the 1720s and 1820s. The framework is flexible enough to allow for detailed accounts of certain mythmaking texts and of internal intrigues within the societies. Intermittent reviews of political events are also offered, in order to deflate certain myths and to uncover the "real" factors at work.

The book yields some fresh data based on mythmaking sources, but, for the most part, Roberts pieces together secondary accounts. His

lightly sprinkled footnotes offer inadequate guidance to the works he uses. Historiographical analysis is needed but is not supplied. The absence of a bibliography and the meager index are also major flaws. Still Roberts merits praise for mastering a formidable body of literature. He cannot lay claim to many "unexplored areas" but his book surveys more ground than any other comparable work I have seen. Despite its broad coverage, however, it falls short of fulfilling the author's laudable aim to block out "the subject as a whole."

For one thing, the professional blinkers worn by the specialist in modern European history are never set aside. One has the impression that pertinent studies by Simmel, Hofstadter, and even Georges Sorel were excluded as "outside reading" from the start. Political events are emphasized; social and intellectual dimensions are ignored. Relevant issues pertaining to the powers of the press, to "reading societies," and the clandestine book trade are never raised. Furthermore, given a subject that is expansive and cosmopolitan, Roberts adopts a staunchly insular approach. English Masonry is seen to cross the Channel (and become subject to foreign delusions), but it is also arrested at the ocean's edge. Thus Franco-American interchanges are not taken into account. English Masonry moreover is severed from earlier Continental influences. There is no glimpse of the rich Rosicrucian background amply furnished by Frances Yates, and no hint of the conspiratorial mythmaking that flourished under Louis XIV.

By beginning with convivial gatherings in early Hanoverian England, Roberts seems to have chosen the wrong point of departure for viewing his topic as a whole. His ending with "the collapse of the whole world of the secret societies" after the exposure of one of Buonarroti's networks is also open to question. Blanqui and Barbès would be surprised to learn that secret societies thereafter had "real substance only in . . . backward Balkan states." If Roberts had carried his story beyond the fiasco of 1823, at least as far as 1848, then he might be less inclined to dismiss the fears of nineteenth-century statesmen as "baseless rubbish."

Overreacting perhaps to long study of a

lunatic fringe, Roberts uses terms like "rubbish" incautiously and is too quick to cast aspersions on others' mental health. He often invokes a consensus among all sane historians and implies his subject was dismissed as "non-sense" long ago. Yet he himself works at deflating certain myths in a way that suggests they have not all collapsed. He fails to exercise discriminating judgment and rarely airs issues that are subject to dispute.

There is no consensus at present about how seriously one ought to take the plots and police reports with which Roberts deals. Conflicting verdicts have been rendered by several of the authors he cites. Given problems posed by conspiracy, no verdict can be taken for granted. Even skeptics are sometimes deceived. Too often the skeptical author of this book seems to land in the same position as the true believer. He leaves little room for other scholars to differ and allows no margin of uncertainty for himself.

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BÉLA KÖPECZI. *La France et la Hongrie au début du XVIII<sup>e</sup> siècle: Étude d'histoire des relations diplomatiques et d'histoire des idées*. Budapest: Akadémiai Kiadó. 1971. Pp. 624. 41 plates. \$20.40.

This learned and important study presents the interpretations of Hungary's leading Rákóczi scholar, Professor Béla Köpeczi of the University of Budapest, on the diplomatic and political relations between Hungary and France at the time of the great social and national uprising, led by Prince Francis Rákóczi, from 1700 to 1715. These interpretations are based on numerous monographic studies and scholarly investigations in the Hungarian National Archives and the Széchenyi National Library in Budapest, the Archives du Ministère des Affaires Étrangères and the Bibliothèque Nationale in Paris, the Czartoryski Library in Cracow, and the archives of Dresden.

The first part of the study constitutes the author's most important contribution to scholarship. It examines the origins, extent, and course of Franco-Hungarian diplomatic cooperation during the War of the Spanish Succession. This initiative played a considerable role

in sustaining the socially, religiously, and constitutionally inspired Hungarian movement of Rákóczi against Habsburg domination, while it also benefited France by creating a serious military diversion for her chief opponent. Professor Köpeczi's detailed study clarifies several aspects of the Franco-Hungarian relationship. One of these is the now-established fact, formerly disputed or unknown, that Prince Rákóczi requested French support and that Louis XIV agreed only reluctantly to aid the Hungarians. The author describes the efforts of sympathetic Polish noblemen, of the Marquis du Héron, the French envoy in Poland, and of Count Miklós Bercsényi, Rákóczi's deputy, to persuade the French king of the seriousness of the impending Hungarian uprising. Louis XIV agreed to support the prince only after a widespread peasant revolt swept northern Hungary and Rákóczi assumed command of a general military insurrection. Another clarification concerns the refusal of Louis XIV to sign an alliance with the Hungarians. Prince Rákóczi repeatedly sought to obtain an alliance, believing that only French support could sustain his movement and assure Hungarian participation in the peace negotiations. His most controversial act, the deposition of the Habsburg monarch at the Diet of Ónod in 1707, was also motivated by this objective, but the French king refused even then to accept a treaty. The author discusses another interesting diplomatic move, the conclusion of an alliance between Peter the Great and Rákóczi in 1707, through which the enterprising prince hoped to gain international recognition for an independent Hungary or Transylvania.

On the basis of these and other conclusions the author presents several reinterpretations of Rákóczi's politics and diplomacy. He sees the prince, contrary to earlier views, as an enterprising, imaginative, and realistic diplomat who consciously pursued the goal of a self-governing Hungary or Transylvania. Professor Köpeczi argues that the idea of an independent Transylvania represented a realistic alternative to membership in the Habsburg Empire. In his view, Rákóczi achieved important financial and diplomatic support from France and thus made possible a Hungarian role in European politics. Furthermore, his proposal of a Franco-

Russian alliance suggests his understanding of European diplomatic alignments.

Space limitations forbid an appraisal of these viewpoints. In any case, they represent stimulating reinterpretations worthy of further exploration. Furthermore, the author's sustained narrative of Franco-Hungarian relations represents an original and valuable contribution to European diplomacy. Its great merit is the clarification of Rákóczi's diplomacy in relationship to European international politics, the politics of Habsburg absolutism and eighteenth-century European history.

In the second part of his study the author examines a somewhat related theme, the reception of the Rákóczi uprising by French public opinion and the significance of French opinion on Hungary in the evolution of eighteenth-century political ideas. While an immense quantity of useful information is cited here, the attempt to relate these data to the general theme is only partially successful. The lengthy and amorphous information of this section is not sufficiently analyzed and integrated with the author's overall theme. Nevertheless, the conclusions, in which he points out the primacy of political considerations in judgments of the role of Hungary by Western thinkers, are valid and suggest important viewpoints. So, too, is the final conclusion that Rákóczi as a diplomat and as a representative of an emerging enlightened absolutism made a notable impact on eighteenth-century European politics. For these reasons the study as a whole is recommended as the most authoritative presentation available in a Western language on the political role of the Rákóczi uprising. The scholarly value of this publication by the Hungarian Academy is enhanced by an index of names, forty-one illustrations, careful footnoting, and a historiographical essay.

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MARIO D. FENYO. *Hitler, Horthy, and Hungary: German-Hungarian Relations, 1941-1944*. (Yale Russian and East European Studies, 11.) New Haven: Yale University Press, 1972. Pp. xii, 279. \$10.00.

Any historian seeking to do original work on the political and diplomatic history of Hun-

gary between 1918 and 1945 finds himself struggling to uncover evidence and suggest insights that were not already provided by C. A. Macartney in his unique and indispensable study of the period. Probably no historian can ever hope to match Macartney's intimate knowledge of personalities and events in Hungary during the 1930s and 1940s. It is thus a tribute to any author, in this case to Professor Fenyo, when he is able to produce a study that improves significantly on portions of Macartney's account.

This monograph, which concentrates on German-Hungarian political relations during World War II, introduces important new source material relating to a number of crucial episodes. In some cases, such as the knotty problem of the origins of the Kassa bombing incident of 1941 or Horthy's conversations with Hitler at Klessheim Castle in 1944, the author carefully sifts old and new evidence only to conclude that we may never know the true story. On other matters, however, he is able to draw more definite conclusions. For example, a close examination of Kállay's ambiguous policies leads Professor Fenyo to suggest that the Hungarian prime minister was hardly the hero of anti-German resistance that some writers have portrayed.

The book's greatest strength is the broad range of unpublished sources on which the narrative is based. The author demonstrates an enviable command of the German, Italian, American, and Hungarian documents found in the National Archives. However, it is to be regretted that the records of the British Foreign Office, now open to 1945, could not be consulted, for London was unusually well informed about Hungarian affairs, and the British played a prominent role in negotiations for a separate peace.

Professor Fenyo's work will be of great value to students of East Central European history, but it probably is not suitable for nonspecialists. Little attempt is made to provide introductory information: the political, economic, and social structure of interwar Hungary is only briefly discussed in somewhat disjointed flashbacks. A brief concluding chapter does not deal in depth with some of the more intriguing questions raised in the narrative, such as the nature of possible anti-German sentiments

among Hungarian peasants or the reasons for the prevalence of fascist and pro-German sympathies among army officers. It is to be hoped that Professor Fenyo in future studies will turn to a careful analysis and interpretation of the history of the German-Hungarian relationship that he has here so skillfully reconstructed.

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LAWRENCE STONE. *The Causes of the English Revolution, 1529-1642*. New York: Harper and Row. 1972. Pp. xiv, 168. Cloth \$12.50, paper \$2.95.

G. E. AYLMER, editor. *The Interregnum: The Quest for Settlement, 1646-1660*. (Problems in Focus Series.) [Hamden, Conn.:] Archon Books. 1972. Pp. viii, 248. \$10.00.

CHRISTOPHER HILL. *The World Turned Upside Down: Radical Ideas during the English Revolution*. New York: Viking Press. 1972. Pp. 351. \$10.95.

These three books are the work of eleven well-known students of the English revolution, an event that has, in the last thirty years, attracted more interest than any other period of English history. The authors share some excellent qualities: they are immensely learned; they write well; and they transcend the sterility of mere pedantic learning and lead us into a world of good judgment, delicate sensibility, and a concern for important human values. In addition, their personal biases are as candidly stated as one could wish. The three books are models of contemporary historical writing.

The authors are in considerable agreement on many of the basic questions. They all think of the events of 1640-60 as a "revolution," they describe the course of the revolution in the same tone, and they give generally compatible accounts of its causes and results. Yet they are careful to acknowledge that other scholars of equal learning and judgment, but with other points of view, would dispute most of their conclusions; for example, Professors Elton and Trevor-Roper and Mr. Laslett, for different reasons, would not agree that this was a "revolution."

Although all of these scholars have contributed to that recent and remarkable explosion of knowledge about the basic facts of the pe-

riod—"what really happened"—they do not look forward to a time when the sheer weight of established fact will eliminate differences of interpretation. Historians, they seem to assume, are humanists whose notions of what is important in history will continue to vary and whose debates, consequently, will never be concluded by additional evidence. Additional evidence will, of course, alter judgments. And the knowledge that judgments are never final may induce a humility that will eliminate such arrogant incivility, for example, as Professor Trevor-Roper's scurrilous attack on R. H. Tawney. The authors of these three books are urbane, civilized, and modestly tentative.

Professor Stone's book contains one major interpretive essay, "The Causes of the English Revolution," and two short historiographical essays, "Theories of Revolution" and "The Social Origins of the English Revolution." In "Theories of Revolution" he makes a determined effort to be kind to the social scientists who have written on the subject. He discusses "situation[s] of multiple dysfunction exacerbated by an accelerator," "dissensus," "J-curve theories," and the like, but he does not demonstrate that these are of any practical use to historians. Indeed, he himself warns us against this "obfuscating jargon, much of which conceals solemn statements of the obvious."

But if his essay on theory is unconvincing, his practice as a historian proves him at least partially right: he has made exemplary use of the methods of measurement, systematic sampling, and statistical testing that the social scientists have perfected.

"The Social Origins of the English Revolution" opens with a judicious summary of the controversy over the "rise of the gentry," the most famous historical controversy of the last half century and one to which Stone himself contributed with distinction. The questions raised are not yet answered and are, perhaps, not even the right questions. But Stone indicates that as a result of painstaking research and imaginative generalizing, "truth—partial, imperfect, provisional truth—is starting to emerge." "Looking back at the controversy today, it seems clear that all parties . . . failed to see that revolutions have extremely complicated origins, and that social causes are only one among many." "A more sophisticated view

of the causes of the English Revolution is beginning to emerge."

"The Causes of the English Revolution" contains both a concise review of previous interpretations and a catholic and magisterial synthesis of his own. Starting with the reasoned presupposition that this was a real "revolution," although not a Marxist one of class conflict, he divides the causes into "the preconditions, 1529-1629," "the precipitants, 1629-39," and "the triggers, 1640-42."

The preconditions include such long-term causes as the weaknesses of Tudor policy, the doubling of population between 1520 and 1640, and many others, all subtly woven together to demonstrate how everything affects, and is affected by, everything else. The preconditions made some redistribution of power almost inevitable, but whether by peaceful or violent methods depended upon the actions of government and opposition. The precipitants, a series of short-term developments such as Laud's ecclesiastical policy, brought the collapse of government from "the realm of possibility to that of probability." Finally, the "triggers" were "a series of unfortunate accidents and misguided personal decisions" that made war inevitable.

Professor Stone's analysis is balanced and informed. Another of his strengths is his candid admission that "every historian is obliged to follow his own judgement in the role he ascribes . . . to accident and to individual personality." He must ask himself what would have happened had so-and-so acted differently: "no logical scientific defense can be offered for such a procedure, but merely the weak argument that it seems to make sense to the working historian." And to the reader, may I add, when the historian is Professor Stone. But again in this essay, his references to the abstractions of the social scientists do not strengthen his analysis: the quotation from Chalmers Johnson that "revolution is always avoidable if only the creative potentialities of political organization can be realized" is a pompous truism.

Professor Stone's study ends with some interesting remarks about the significance of the English revolution. Its lasting importance lies in "the intellectual content of the various opposition programs and achievements after 1640. . . . For the first time, there came on the stage of history a group of men proclaiming liberty

not liberties, equality not privilege, fraternity, not deference. These were ideas that were to live on, and to revive again in other . . . ages."

"It is this legacy of ideas which makes it reasonable to claim that the crisis in England in the seventeenth century is the first 'Great Revolution' in the history of the world, and therefore an event of fundamental importance in the evolution of Western civilization."

*The Interregnum* contains eight essays and a thoughtful and graceful introduction by the editor. The nine authors agree that this was truly a revolution, although, as Aylmer points out, they might have a hard time arriving at a common definition of the term. All agree that the Restoration was not inevitable: long-term trends worked in favor of a return of the old order, but it might not have come about except for some of those adventitious events which Stone calls "triggers."

The most interesting of these essays—that by Quentin Skinner—is a wholly convincing proof that Hobbes was not an isolated and eccentric thinker, but one who dealt with precisely those problems of political authority that were of most immediate concern to his contemporaries. Hobbes's distinction is that he was a profound philosopher who saw infinitely deeper than the pamphleteering propagandists who dealt with the same questions. In this brief, learned, and elegant essay, Skinner shows us the way of ideas in history and the relation between genius and its time.

Keith Thomas's critical examination of the thesis that the Levellers were not democrats but champions of property rights settles that issue. But he goes astray, I think, when he says that the Levellers' "economic program was backward-looking" because they wanted to create a society of small, independent proprietors: Thomas Jefferson and those of his successors who gave away quarter-sections of the public land to all comers and who wanted to give every freedman forty acres and a mule were not reactionaries, although they may not have been riding the wave of the future.

The essays by Valerie Pearl, Claire Cross, J. L. Cooper, and Ivan Roots examine the role of London in the Interregnum, and various aspects of Cromwellian policy. Professor Underdown gives a concise summary of the Court-Country tensions and generalizes in an



interesting fashion about the tendency of modern revolutions to nationalize and to destroy provincial loyalties. Finally, Professor Woolrych describes the last attempts (1657–60) to achieve a settlement without Charles II. None of these, he says, was necessarily unworkable, but the “folly and inadequacy” of the leadership made them unworkable.

Christopher Hill is the master historian of his chosen field—“Hill’s half-century.” In this dazzling new book, his best work so far, he demonstrates again his enormous capacity for painstaking research, his fertile, warm, and sympathetic imagination, and his command of an English style exactly suited to his purpose.

The leaders of the English revolution did not worry about the welfare of the lower half of the population, but with the collapse of the old order there came a brief moment of “glorious flux and intellectual excitement” when spokesmen for that lower half called in question every traditional belief and ancient institution. Hill looks at the most radical critics of the day, the “lunatic fringe,” in the conviction that they “have something to say to our own generation,” which is coming to understand that “madness itself may be a form of protest against social norms, and that the ‘lunatic’ may in some sense be saner than the society which rejects him.”

This is a guide book for the Island of Great Bedlam—inhabited by Familists, Seekers, Diggers, Grindletonians, Muggletonians, Millenarians, Quakers, Baptists, Fifth Monarchists, Ranters, and others without a name. The author’s learning is astonishing: we have known something about some of these people, but Hill has dug up dozens of unknown radicals and radical pamphlets dedicated to turning the world upside down.

The opening chapters summarize the changes in the century before 1640 which gave birth to the suppressed lower-class radicalism that burst into light during the anarchic days of the revolution. Hill acknowledges that we need to learn more about the origins: here is a rich field for future diggers. Chapter 7 introduces us to the hero of the work, the Digger Gerrard Winstanley, and later chapters describe the ideas of a number of lesser-known radicals.

The variety of radical opinion that Hill examines does not fit together as a consistent

social philosophy. But he demonstrates that by selecting, we can find in it anticipations of most of the radical ideas of today. These thinkers were mostly egalitarians and democrats; some wanted not only communal ownership of land but collectivized, large-scale, scientific agriculture; some were rationalists who rejected the Bible, heaven and hell, and the central dogma of Puritan Christianity, original sin (“no damn merit in salvation”); some attacked traditional learning and science and the universities as supporters of the oppressive status quo and found alternatives in astrology and alchemy; a few preached sexual equality and sexual freedom; others were anti-imperialist and nearly all distrusted military, legal, and political establishments; finally, many of them rejected the ethic of work which Hill (the hardest working of historians) calls Protestantism’s chief contribution to the dark world of capitalism.

We can glimpse in this fragmented radical literature a new culture or counter-culture that rejected “private property for communism, religion for a rationalistic and materialistic pantheism, the mechanical philosophy for dialectical science, and asceticism for unashamed enjoyment of the good things of the flesh. . . . Its ideal would have been economic self-sufficiency, not world trade or domination.” The Puritan emphasis on sin led to the ethic of work, the compulsion to labor, to save, to accumulate. While some of the radicals simply rejected this ethic, Winstanley suggested an alternative: exploitation, not labor, was the evil; abolish exploitation, and labor to beautify and furnish the commonwealth would be a pleasure. “Coolly regarded,” Hill says, “we must agree that this was never more than a dream: the counter-acting forces in society were too strong.” But then he adds, “it came nearest to realization in the Digger communities which might have given the counter-culture an economic basis.” Brook Farm?

These obscure radicals, Hill claims, sketched for us “a possible society which would transcend the property system, of a counter-culture which would reject the protestant ethic altogether.” We might be grateful to those men “who foresaw and worked for not our modern world, but something far nobler, something yet to be achieved—the upside-down world.”



As he comes to the end of this remarkable book, the reader may forget that he has been reading a history: he will have heard a richly orchestrated *Te Deum*, an epic oratorio celebrating the triumph and tragedy of the lunatic fringe. "Their poetic insights are what seem to me to make them worth studying today." The reader's judgment of the merits of the book will be influenced by his taste in poetic insights.

The reader who is also a historian will have some questions. Did these madmen have any chance of succeeding? Hill says that they did not; he agrees, with all the other historians we have been discussing, that the historical tide was running the other way. But like them, he is also aware of the contingent and unexpected in human affairs: we would not expect President Nixon to invite Abbie Hoffman to the White House for a serious talk, but, as Professor Aylmer points out, Cromwell did just that for George Fox, who was as wild as any yippie. Still, Cromwell did not take Fox's advice.

Were the radicals a large and influential group? Hill agrees that they were a small minority, but he has demonstrated that there were more of them than we had previously imagined. Moreover if we may assume that they came from social levels normally illiterate, we may think of them as the visible tip of a massive, hidden iceberg. This raises the question of what happened to the iceberg after 1660—but that is not Hill's problem.

Did the ideas of Hill's radicals live on to inspire later generations? He agrees that they were pretty much forgotten, but he has dug up some evidence to show that more lived on in Milton, Bunyan, Blake, and the radical underground than we have hitherto suspected. But in the end these radicals are influential today, not because they survived but because they have been revived—by Hill.

In so rich and varied a book the critical reader will find many judgments to challenge. But the basic question is, what are we to think of Hill's picking and choosing from the writings of his radicals, not by any criterion which they would have used, but by his own criterion of what he judges useful today? He acknowledges that he has "picked out the most favorable examples. . . . a lot of nonsense was

talked and written." Surely the result of his method is to give us a badly skewed picture of seventeenth-century radicalism.

To which Hill might well answer that he is not interested in an exact rearticulation of the skeletons of dead controversies. He wants to show the present generation how the revolution liberated "the imagination as Christ rose, however briefly, in sons and daughters." He concludes with some reflections on the writing of history.

"The radicals assumed that acting was more important than speaking. Talking and writing books, Winstanley insisted, is 'all nothing and must die; for action is the life of all, and if thou dost not act, thou dost nothing.' It is a thought worth pondering by those who read books about the seventeenth-century radicals, no less than by those who write them. Were you doers or talkers only? Bunyan asked his generation. What canst *thou* say?"

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WILFRID R. PREST. *The Inns of Court under Elizabeth I and the Early Stuarts, 1590-1640.* Totowa, N.J.: Rowman and Littlefield. 1972. Pp. xii, 263. \$13.50.

Mr. Prest's book is one of those rare first-books-from-thesis that really meets the standard so nicely put in the Oxford statute for the D. Phil., "an original contribution to knowledge in fit form for publication." It is the first full-scale study of the Inns of Court for any period to appear for many years, and its scholarly quality is measurably superior to all previous studies. It sets a high mark for subsequent studies to attain. Those studies are not far away: Walter Richardson is completing a book on the Tudor Inns of Court and a young scholar at Newfoundland, Thomas Evans, is working on the Restoration Inns. Mr. Prest's book also advances the laborious task of understanding the Tudor-Stuart legal profession, work that has engaged the efforts of Eric Ives and, latterly, Louis Knafla and William Jones. It will stand as a pioneer study in what is becoming a major scholarly assault on the enigma of the early modern common law.

Approximately one-half of the book deals

with the structure, the administration, the finances, the composition (benchers, readers, utter-barristers, students), the maintenance of order, and the pedagogical function of the Inns. This is finely detailed analysis, drawn from a wide range of sources including the muniments of the Inns, commonplace books of students and barristers, memoirs, and the growing literature of the early seventeenth century on legal education. Pains taken to deal quantitatively with status and regional origins of entrants, numbers of calls to the bar and the bench, continuance at calls, attendance of benchers, and incidence of casual violence result in clear and useful tables. It is more a quibble than a criticism to question whether the entrance statistics based on years or moving averages of years were uniformly rectified to a calendar year beginning January 1. The statistical data are never allowed to serve for qualitative analysis and description, and the chapter on "Learning the Law" is a model for the history of pedagogy. For the first time we are afforded a concise, comprehensive description of how a barrister learned the law, with full recognition of how the learning process changed in the period to the point of virtual disappearance by the end of the seventeenth century. This chapter and the one following, "Legal and Liberal Education," bury the myth of the Inns as the "third university" of Tudor-Stuart England. Mr. Prest establishes beyond question that the Inns' pedagogic function, already in decline, was aimed wholly at training professional lawyers, not teaching law to gentle scions. Yet, perhaps he concludes too severely that sojourn at an Inn was largely formally noneducative for the casual student. There was a wide gap between the benefits rather shamelessly touted by the Inns (who did well by the gentle horde) and the benefits that actually accrued to the sojourner. But the more we learn of litigation in the period the more apparent becomes the "learned lay client," the landed litigant sufficiently knowledgeable about the law to instruct counsel directly and intelligently. If the growing body of legal literature was the principal source of his knowledge, a year or two at an Inn probably contributed to his ability to use that literature. This, however, demands more study, and the onus is now on the advocate of this position to

prove the educative value of the Inns of Court to the casual student.

The last three chapters, two of them dealing with the religious complexions of the Inns and the last chapter on "The Inns of Court and the English Revolution," are provocative and yet inconclusive. Mr. Prest's treatment of Puritanism is a long-needed corrective of the excesses of Eusden's thesis, and a short chapter on "Papists" is a valuable contribution to recusant history. The final chapter demonstrates once again the problem of connecting Stuart professional experience with revolutionary ideology, a problem that Gerald Aylmer encountered in his study of Caroline bureaucracy. Mr. Prest's problems in these chapters have less to do with shortcomings of research (there are none) or of conceptualization (this is remarkably sophisticated) than with our continuing ignorance of lawyers in their professional roles. Indeed, this lacuna haunts the book and the work of us all in Tudor-Stuart legal history. Mr. Prest is thrown back on a list of practicing counsel, numbering 489, drawn from appearance in the reports for the period 1590-1640. My own research on counsel in the Star Chamber indicates almost 1,500 counsel signed pleadings in that court, 1603-25, comprising virtually the entire bar in that period. Mr. Prest shows some ambivalence in assessing the importance of the Inns as the principal vehicles for conferring professional recognition upon practicing barristers over the course of their careers precisely because we know very little about the economic dimension of legal practice and even less about how a barrister's practice was begun, advanced, structured, and founded.

If the next step is a detailed and comprehensive study of the legal profession, Mr. Prest has pointed to areas of investigation that cannot be ignored in that task. The role of the Inns' benchers in the practicing profession is a central concern; if the readership had lost its practical distinctiveness, the status of bencher perhaps still had a critical influence on professional weight. The political dimension of being a lawyer obviously turned on the particular Inn, and we might gain something by talking less about "lawyers in Parliament" than about the "Lincoln's Inn saints in Parliament." Mr. Prest has provoked these random notions. With

a long and productive career yet ahead of him, he will doubtless raise other, more fundamental, questions and likewise answer them. He has made a bold start.

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RAYMOND H. LOUNSBURY. *Pennoyer Brothers: Colonization, Commerce, Charity in the Seventeenth Century*. Philadelphia: Dorrance and Company. 1971. Pp. xiv, 237. \$5.95.

Dr. Lounsbury's book is a filiopietistic work of genealogy, which was a very popular genre in the nineteenth century but has few practitioners at the present time. As a descendant, evidently, of Robert Pennoyer, he has collected most of the extant evidence about that colonist's altogether undistinguished career. Dr. Lounsbury was more attracted by Robert's elder brothers William and Samuel, who cut an important figure in Commonwealth government and trade policy and from the former of whose will the author seems to have been a beneficiary. The text and administration of William Pennoyer's will take up one-quarter of the book. It is always helpful to have primary sources printed, and the details of administration are instructive as to seventeenth-century practice. I share Dr. Lounsbury's belief that the Pennoyers have been unduly neglected as historical figures, but the present work does not remedy the deficiency. Virtually all his sources are printed ones, and much of the argument is derived from previous authorities such as V. T. Harlow and W. E. Foster. Although Dr. Lounsbury consulted a few English Chancery records, mostly in connection with the settlement of William Pennoyer's will, he missed the Chancery sources on William and Samuel's pre-1640 career in the Levant trade, which had an important bearing on their later politics and commerce. Such research has been done by others, such as Robert Brenner, who place their findings about the Pennoyers in a much more impressive interpretive framework than the trite generalities that characterize this book.

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B. S. CAPP. *The Fifth Monarchy Men: A Study in Seventeenth-Century English Millenarianism*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 315. \$18.50.

In a slightly modified version of his doctoral dissertation (Oxford, 1970), B. S. Capp has written the best analytical study of the Cromwellian Puritan sect, the Fifth Monarchy Men. It surpasses the sixty-year-old political study of Louis Fargo Brown as well as the recent narrative treatment by P. G. Rogers. Capp's book, however, does not achieve the theological insight of Alfred Cohen's doctoral dissertation, "The Kingdom of God in Puritan Thought: A Study of the English Puritan Quest for the Fifth Monarchy" (1962).

Capp is at his best when he analyzes the distribution and composition of the Fifth Monarchy Men. Two extremely useful appendixes, one identifying some 280 Fifth Monarchy Men by name and the other giving the locations of some seventy congregations and groups, enable Capp to say that the Fifth Monarchy sect, unlike the Quakers who were drawn from the country and from a wider social range, was an urban movement that attracted "the very bottom strata of society" (p. 85). The influence of a strong local leader, not an economic crisis, was an important circumstance in the rise of a Fifth Monarchy group, and nearly one-third of Capp's 280 men served as officers or rank and file in Cromwell's army, where millenarian ideas were rampant.

Capp is on sure ground when he says that "millenarianism . . . was indeed the *raison d'être* of the movement" (p. 14) and that the "Fifth Monarchists were unanimous that the saints should reign with Christ their King" (p. 137). The major differences were over whether Christ was to come in person and whether Christ needed any help from the saints. Capp is surely right when he says that Fifth Monarchism had a political program that was elitist in the rule of the elect over the ungodly, a social program that was egalitarian within the ranks of the saints, an economic program that was not anticapitalist, and a program for law reform that was based upon the Mosaic Code.

In the absence of a declaration of faith similar to those produced by the Independent and Baptist churches, it is difficult for Capp to

identify a theological position for the Fifth Monarchists, but he does note their combination of "biblical literalism and inward inspiration" (p. 188). Although he states that the Fifth Monarchy Men's devotion to the Mosaic Code made them hostile to Antinomianism, he observes that several of them had advocated Antinomianism in their earlier years, perhaps when they were imbibing its mystical ideas, together with millennial ideas, in Cromwell's army. Professor Cohen has suggested that Antinomian theology was closer to the internalized, Spirit-centered, and passivist kind of millenarianism that was to become so characteristic of the Quakers, whereas the externalized, Christ-centered, and activist kind of millenarianism was closer to the Covenant theology of such Fifth Monarchist leaders as Vavasor Powell, William Aspinwall, and John Tillinghast. Indeed, those very men whom Capp cites as having influenced Fifth Monarchy ideas of law reform (William Perkins, John Cotton, and Hugh Peters) also advocated Covenant theology. The tension in Covenant theology between man's fragile capability and God's overwhelming design, which was less present in deterministic Antinomianism, is a possible theological explanation for the reluctance of nearly all of the Fifth Monarchy Men, despite their seditious rhetoric against Cromwell's government, to take up arms against the Protectorate without a sure sign of the impending millennium.

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JOHN KENYON. *The Popish Plot*. New York: St. Martin's Press. 1972. Pp. 300. \$10.00.

This book is attractively produced with seventeen illustrations well chosen from contemporary prints and portraits. Unfortunately the notes are inconveniently placed at the end (pp. 276-91). The author is well known for his excellent life of *Robert Spencer, Earl of Sunderland, 1641-1702* (1958). The Popish Plot in one or another aspect has been treated in an interesting novel by John Dickson Carr, and in a brilliant exposition, particularly of the criminal procedure of the times, by Sir John Pol-

lock, as well as in other articles and monographs, to which Kenyon refers and with some of whose conclusions he differs. Pollock, he feels, was still influenced by the reign of Edward VII—that is, somewhat anti-Catholic in his judgments about the mysterious murder of Sir Edmund Berry Godfrey (p. 266). Kenyon relegates to an appendix a murder he thinks may have had little to do with the "Plot," though its effects upon public acceptance of the stories of Titus Oates and William Bedlow were so important. Kenyon's emphasis is less exclusively upon the effect in the capital of the mass hysteria of 1679-81 that sent so many Catholics to an undeserved death than upon the situation of the papists in the country at large before 1678 and the effects of the plot upon them thereafter.

The usefulness of *The Popish Plot* will be found, not so much in the ways in which its judgments of events and people differ in detail from earlier writers, but in the care with which the author has examined the records in provincial England, as well as in London, and the estimates arrived at as a result as to numbers, strength and weakness, treatment by neighbors, and decline of the various families, communities, and regular and secular clergy comprising the Catholic population of the reign of Charles II. Noblemen in most cases, save for Viscount Stafford, and a few self-exiled, protected by rank and often tolerated by their fellow gentry, nonetheless felt frustration at being excluded from the normal public service of their class. As many as ten members of a family of eleven took vows of celibacy in one instance; the frequency of this occurrence was, perhaps, one cause for a diminishing Catholic population. The plot itself dealt a great blow to the Jesuits, most hated of orders, but also to Benedictines, Dominicans, and schools maintained abroad by the religious for the education of English children. The Glorious Revolution, though it took fewer lives and could boast of no Oates, resulted in further weakening of their position. Some conformed; other families died out. There remained a substantial residue after the shouting had died down, to renew the faith when more tolerant days arrived.

Mr. Kenyon is judicious and careful. He has done a great deal of work for this book and has studied those problems connected with the his-

tory of the Popish Plot, which have until now received comparatively little attention.

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JOHN CRESWELL. *British Admirals of the Eighteenth Century: Tactics in Battle*. [Hamden, Conn.:] Archon Books. 1972. Pp. 263. \$13.50.

This book surveys a century of British naval tactics from Malaga to Trafalgar. Its main concern is to show why the line-of-battle formation remained the basis of tactical doctrine throughout the period. Captain Creswell makes a persuasive case. He displays the powerful advantages of fighting in line, as well as the hazards that were inherent in any attempt to achieve a concentration of force on a portion of a well-ordered enemy line (in fleet engagements one could not count upon sufficient speed and deception to bring it off). "Apart from hard fighting," he notes, "there could only be the hope that the enemy's commander-in-chief would so misconduct his line as to give you an opportunity of bringing such a concentration to bear." Finally, we are made to see that practically every admiral of the eighteenth century recognized all this. In short, the book is a response to those naval historians—A. T. Mahan, Sir Julian Corbett, indeed, almost everyone—who have supposed that Lord Nelson's predecessors might have made short work of their opponents, if only they had discarded the formalism of the fighting instructions in favor of the bold initiatives that marked Nelson's style. But, as the author points out, Nelson had good reason to be confident that his opponents were incapable of reacting quickly enough to frustrate his plans. Such confidence was seldom justified during most of the eighteenth century.

For the general historian the book's first three chapters are likely to be the most valuable. They offer a brief, clear explanation of the logic of tactical development and its relation to the technical characteristics of the ship of the line. Even readers whose knowledge of seamanship is feeble—there are some helpful asides for their benefit—will grasp the main points without much difficulty. In the remaining chapters, which deal with the significant battles of the century, the author amply illustrates his argument that the Printed Fighting Instructions were neither permanent nor imposed

upon fleet commanders by Admiralty authority. The notion that officialdom stifled initiative and innovation is false. Any commander in chief could issue his own Additional Instructions to his captains, and many did so. All in all, what emerges most strikingly from the author's authoritative commentary on these battles is that British failures arose less often from flaws in the instructions than from their improper execution and a lack of aggressive pursuit of an enemy in disarray.

Captain Creswell does not exhaust the subject; his study focuses on what occurred when two roughly equal fleets engaged and does not consider how changes in the logistical balance may have affected tactics under other circumstances. His concluding discussion of British and French gunnery skill is somewhat puzzling and unsatisfying. And one could wish that in his battle commentaries he had kept his main themes more plainly in view. Nevertheless, this is by far the best informed and most judicious study of eighteenth-century tactics that exists. Anyone who ignores it in favor of the orthodoxies propounded eighty years ago is likely to be wrong.

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R. S. NEALE. *Class and Ideology in the Nineteenth Century*. Boston: Routledge and Kegan Paul. 1972. Pp. viii, 200. \$9.25.

This slim volume is a collection of seven articles, chiefly on Victorian social history. Among them are "Class and Ideology in a Provincial City: Bath 1800-50," based on the author's M.A. thesis at the University of Bristol; a review of John Vincent's monograph on poll books; and "The Colonies and Social Mobility: Governors and Executive Councillors in Australia, 1788-1856." Since most of the articles have already appeared in print, the reason for assembling them in book form is not immediately apparent.

Presumably, the justification for the book is to be found in the first essay, "Class and Class Consciousness in Early Nineteenth Century England: Three Classes or Five?" In it Neale argues that historians have been misled by the "three-class model of social structure in the early nineteenth century," in which individuals are placed in the aristocracy, middle class, or



working class. He suggests that it be replaced by a five-class model: upper class, middle class, middling class, working class A, and working class B. In an introduction written for this volume, the author explains that all of the articles except one have been presented within the framework of the five-class model.

Unfortunately, the essay that forms the centerpiece of the book leaves a great deal to be desired. For one thing, Neale's target is no more than a straw man at best. Historians have not in fact been taken in by a "three-class model," although they may use that sort of terminology. J. F. C. Harrison, for example, in *The Early Victorians* (London, 1971), emphasizes the extent of stratification within both the "middle classes" and "working classes." In one sense, therefore, Neale is discussing a nonproblem of his own creation. Having set up an artificially rigid "three-class model" as his target, however, he has been trapped into incorporating much the same sort of rigidity into his own "five-class model," which turns out to be a slightly modified version of the original. Thus, Neale's "middling class" comprises "petit bourgeois, aspiring professional men, other literates and artisans." What they have in common is not made clear. In fact, throughout the essay the author flits from topic to topic without providing the analysis required to handle a subject whose complexity he has underestimated.

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GEORGE L. HERSEY. *High Victorian Gothic: A Study in Associationism*. (The Johns Hopkins Studies in Nineteenth-Century Architecture.) Baltimore: Johns Hopkins University Press. 1972. Pp. xx, 234. \$15.00.

STEFAN MUTHESIUS. *The High Victorian Movement in Architecture, 1850-1870*. Boston: Routledge and Kegan Paul. 1972. Pp. xvii, 252. \$19.25.

PAUL THOMPSON. *William Butterfield*. Cambridge, Mass.: M.I.T. Press. 1971. Pp. xxix, 526. \$25.00.

The rewriting of Victorian architectural history continues. There are three accelerating trends. One rejects what Professor Hersey calls "the invective tradition" (p. xviii) in stylistic

evaluation. Another approaches style itself as more than mere description, attribution, and pedigree. Instead "we must try to reconstruct the Victorian vision of the past from pictures and descriptions and then see the Victorians' own buildings and their own choice of historical motifs in the light of that reconstruction" (Muthesius, p. 3). A third trend sets individual architectural effort in a firmer historical and sociological perspective—emphasizing the structure and role of patronage, the economics of the building industry, and the professionalization of practice. These three studies illustrate clearly how these new approaches can transform existing knowledge and former judgments. They also have much to say to nineteenth-century historians whose interest in architecture per se is minimal.

Hersey's basic contention is that the eighteenth-century esthetic doctrine of associationism—the investing of artifacts with anthropomorphic and mechanical properties—was transmitted from its progenitors, men like Ledoux in France and Alison in Scotland, via Loudon, Ruskin, and the Ecclesiologists, into High Victorian Gothic precept and practice, until eclipsed by the new iron and glass technology and Ruskin's retreat from the Gothic. The thesis—a kind of architectural Great Chain of Being—is an exciting one that ought to convince, given Hersey's very wide reading and evident sensitivity to Victorian architectural language, both verbal and visual. Yet it does not. One problem is that the style—a hectoring compound of Arnold Hauser and the later Ruskin—does not engender confidence in the argument. In a sense Hersey's strengths are literary and descriptive rather than historical and analytical (his comment on page 53 on how "the pre-Victorian ruin cult is beautifully discussed by Rose Macaulay" is quite revealing) and often lead to some dazzling and original insights. "For the ecclesiologists the church building was a penitential appliance, an architectural paraphrase of the suffering body of Christ" (p. 68). "Despite his clamorous and witty prose [Pugin] often breaks into setpieces that remind one of Leigh Hunt, Hazlitt, or even Chateaubriand. . . . It would be interesting to investigate the French side of Pugin's intellectual heritage" (p. 66). But there are also too many minor errors. Ruskin was not exactly



"a protege of Loudon's" (p. 23). Butterfield's dislike of hassocks was on democratic rather than puritanical grounds. Those who viewed the interior of All Saints Margaret Street in 1859 were hardly likely to do so with a "Wilkie-accustomed eye" (p. 114) at a time when the High Church laity, among others, was already on familiar terms with the Pre-Raphaelite esthetic. But the central deficiency is methodological. "Buildings," Hersey tells us, "are discussed only insofar as they contribute to the elaboration of my ideas" (p. xix). Hence the main argument never receives the precise handling it deserves. One is never quite clear whether associationism in such a context was an explicit doctrine, a cognitive mode, or a mere stylistic preference. One also wonders whether, utilizing the same sources, a case could be made for the presence—among architects, clients, and public—of a Romantic, Keatsian brand of associationism (where "new works of art are created as poetic responses to existing ones" [p. 260]) in parallel to the Alisonian species. But too often the prevalence and potency of associationism is assumed rather than proved. A more effective way of tapping architectural streams of consciousness might be by a thoroughgoing "structural" analysis involving the comprehensive collection and analysis of key architectural metaphors. Such an analysis might reveal that associationism was but one component of a High Victorian Gothic esthetic that also embraced functionalism, antifunctionalism, neoclassicism, order, and disorder.

Dr. Muthesius, in his closely written monograph, approaches such an analysis by suggesting that for the High Victorian architects, the traditional eighteenth-century esthetic categories of the sublime, beautiful, and picturesque had a significance that was either predominantly formal, psychological, or associational according to the architect, patron, or school involved. "For the Ecclesiologists 'sublime' is not only an aesthetic or stylistic alternative but also a general moral principle" (p. 13). For Pugin and Lamb, the picturesque predominated. In G. G. Scott, whose St. Pancras is brilliantly analyzed here in a single paragraph (pp. 179–80), we discover "the ultimate combination of Pugin's picturesque and Ruskin's sublime." Functionalism in turn was not a technologically induced *deus ex machina*, but evolved,

notably in the industrial or colonial church, from traditional considerations of picturesque utility. Muthesius also shows convincingly how the apparent eclecticism of much High Victorian architecture was not the product of ideological confusion but the result of an increasing exposure of English architects, through published source-books and the Continental railway, to highly selective features of the European tradition. It was France, not Italy, that provided most impetus after the mid-fifties, but as Muthesius points out, in terms of *contemporary* inspiration and example "between the continent of Europe and England there seemed to be very few exchanges either way" (p. 207). There is much else besides. Ruskin's influence on High Victorian architecture was probably less than it was on High Victorian design. In the Home and Foreign Office competitions of 1857, as with the designs for the Houses of Parliament in the 1830s, "decisions were made and influenced by the taste and opinions of a generation before" (p. 162). Proper attention is paid to the emergent sense of division between what Burges called the "art-architects" and the "professionals," and, as Muthesius shrewdly observes, "as to 'professionalism,' in spite of their criticism, the Ecclesiologists showed many symptoms of this development, with their specialization in one type of building and their specialised communication through the journal and in the meetings of the Society" (p. 161). There are weaknesses. Few leads are given, beyond what is already known, concerning the transition from Gothic to Old English and Queen Anne revival in the early 1870s. There is little about patronage. The style is occasionally tortuous and marred by unnecessary repetition (especially where the picture captions merely reproduce, verbatim, what has already appeared in the text). But these are minor blemishes in what is essentially a major re-evaluation of two crucial decades in Victorian architecture.

Dr. Paul Thompson's study, if superficially more restricted in scope than Hersey's or Muthesius's, turns out in practice to be the reverse. "I wish not merely to describe Butterfield's work, but to interpret it; to discover the forces which shaped it, and the meaning which he and his patrons and contemporaries found in it" (p. xxiii). This *verstehen* approach, so unsatisfac-

tory in Hersey's hands, succeeds triumphantly in Thompson's. The book is divided into two sections, one biographical and analytical, the other mainly descriptive, including a complete catalog of Butterfield's architectural works. Both, despite far too many substandard illustrations for a text of such quality, are continuously fascinating. Historians who may wish to fight shy of chapters headed "The Wall," "The Roof," and "Colour" are advised not to, for Thompson invariably discusses the social and historical context of architectural techniques as well as lucidly describing the techniques themselves. For with Butterfield "no other architect so consistently explored both the *material expressiveness* of wall architecture, and its *discipline* through wall planes; and, at the same time through colour, the triumphant joy of *faith*, and through line and pattern, the *insecurity* of an age of doubt and change" (p. 377). Historians will also welcome the full use Thompson makes of primary manuscript sources in creating a depth study of both an individual (devout, authoritarian, and withdrawn) and an architect (highly professional, scrupulous, and elitist). We learn of Butterfield's opposition to Ruskin's and Morris's doctrines of free craftsmanship (although more on his precise attitude to Ruskin would have been welcome), and how, approving of the shift of taste back to English Gothic in the 1860s, he regarded Bodley's All Saints' Cambridge as "one of the few churches in which he could worship" (p. 93). But the real strength of this study lies in the skill with which Thompson, as a social historian, firmly establishes, rather than merely alludes to, the social, economic, and religious context of Butterfield's work. He makes a good case (far more convincing than any associational equivalent) for the way in which the pervasive religiosity of Victorian life sensitized architects to the intellectual doubts, social bias, and doctrinal divisions of the established Church in a manner which compelled some of them to create architectural forms that would both transcend and resolve them. Butterfield's own religious background—Nonconformist forebears and a High Church mother—and attitudes—prayerbook fundamentalism, antiritualism, yet sympathetic to the ideals of the Oxford Movement—was as original and eclectic as his architecture. "Was it indeed," Thompson rightly

asks, "only for its architecture that he modelled his two most famous buildings upon the upper church at Assisi?" (p. 39). There are also important chapters on patronage (where it becomes clear that the bulk of Butterfield's clients were not drawn from an emergent provincial nouveau riche but from a traditional upper class, often kinsmen of local incumbents), on the builders (seven only of whom were responsible for over a third of Butterfield's ecclesiastical commissions), and an outstanding one on historicism. In short, to judge from Thompson's own work in this volume, Victorian architectural history, in its current transformation, is far too exciting a field to be left to the architectural historians alone.

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MICHAEL ANDERSON. *Family Structure in Nineteenth Century Lancashire*. (Cambridge Studies in Sociology, 5.) New York: Cambridge University Press. 1971. Pp. ix, 230. \$16.00.

Michael Anderson's *Family Structure in Nineteenth-Century Lancashire* offers the most outstanding historical analysis of the role of the family in the process of industrialization. While providing an important model for the quantitative analysis of family structures, the book also represents the first successful attempt to infuse statistical data with qualitative judgments of their broader social significance, and to interpret them within a well-defined body of sociological theory. The study presents an analysis of family and kinship patterns of about seven thousand individuals, including textile laborers, small artisans, and shopkeepers, representing ten per cent of Preston's population. It also includes population samples from rural districts in Lancashire and Ireland, from the places or origin of the in-migrants to Preston. The census data are augmented by rich documentary evidence on the entire textile region. Anderson analyzes migration, family structure, and occupational patterns, thus interpreting family behavior in its social and economic setting. The study is dynamic in its emphasis on migration and on the constant fluidity of the population. The role of the family as an agent of continuity and stability is tested in the context of rapid population turnover, changing in-

dustrial conditions, and social and economic crises.

While recent historical studies of the family have focused on either structure or ideology, Anderson carries his analyses on the structural as well as on the phenomenological level. The study gains depth from the author's effort to analyze family relations as one of several possible alternative relationships. Instead of studying the family in isolation he assesses its functions in comparison with public assistance agencies and a variety of other groups, such as neighborhood organizations and workers' associations.

An important contribution of this exciting book to the study of the nineteenth-century family is in its overthrow of the myth of the "isolated urban nuclear family." Most current census-based family studies have confused the "household" with the "family" unit and therefore concluded that urban families were predominantly nuclear and isolated. Anderson refutes this assumption in his demonstration of critical areas where kin relationships were crucial in facilitating adjustment to urban life. The picture that emerges from his involved multilevel analysis is one of a predominantly familistic society where kinship ties offered the most desirable and most satisfying short-term reciprocal relationships in an urban setting.

Although sociologists and historians have traditionally studied the family as a victim of industrialization, Anderson emphasizes, instead, the role of the family as an active agent. While social disorganization theory stressed the disruption and loss of kinship ties through migration, Anderson argues that kinship survived migration and economic crises and continued as the most pervasive tie, as well as the most important sustaining force in mutual aid relationships. Kinship served in channeling chain migration, in finding work, in sharing residences, in support during dependency and old age, in the care of young children of working mothers, and in assistance during unemployment, illness, or the death of the breadwinner. (Quantitative as well as literary evidence bears this out.) Kin relations were preferable to other organizations or reciprocal institutional relationships because in a fluid society kin provided a more reliable chain of immediate assistance than neighborhood associations or

public welfare organizations.

The socioeconomic model that Anderson constructs for the interpretation of the role of kinship in periods of rapid social change is particularly refreshing in its dynamic view of the family. "Family" is not merely seen as a structural unit, an artifact, or an abstract concept; it emerges as a process that takes different forms over the various stages of the life cycle. "Family" is translated into a series of reciprocal relationships and into various paradigms of exchange and interaction. Kin relations are "functional" and "calculative," based on economic need and rewarded by mutual benefits. While in rural areas family relations are "normative" (sanctioned by the requirements and ideals of a traditional, cohesive society), in the city they are functional and are governed by short-term reciprocal "instrumental" arrangements.

While Anderson's theory provides the most important explanation for kinship and family functions in nineteenth-century society, it also raises some problems, most of which are derived from the intrinsic limitation of available data, rather than from Anderson's theoretical constructs. First, one is not entirely convinced that under the circumstances of rapid population turnover, kin could be a more reliable source of assistance than neighbors. Second, Anderson's interpretation leads one to believe that "affective" relationships (based on emotion rather than on ulterior motive) were secondary in the experience of the working class, especially where critical life situations followed in rapid succession. His model of change in the family over several centuries further deepens the unnecessary dichotomy between instrumental and affective relations. He argues that while preindustrial family relations were normative-based, urban working-class family relations during most of the nineteenth century were predominantly calculative. As working-class affluence increased and as bureaucratized welfare carried some of the burdens of family support, calculative kin relations were replaced by affective relations. The problem with this model is in the assumption that calculative and affective relations are mutually exclusive. This weakness is especially reflected in the chapter on ideology, where Anderson argues that working-class socialization did not instill much love or

devotion to parents, especially not to fathers. In this argument Anderson falls into the trap of the "middle-class" characterization of working-class experience, which he successfully deflates in his book.

Historians of the family who have employed sociological models have often erred on the side of the superimposition of theoretical constructs on a body of historical evidence, or on the marshaling of historical data toward the refutation of a specific sociological theory. Sociologists, on the other hand, have often used historical data to illustrate theory, without attention to the historical context. Anderson overcomes the pitfalls of both approaches in this successful fusion of historical data with sociological theory.

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BRIAN HARRISON. *Drink and the Victorians: The Temperance Question in England, 1815-1872*. [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. 510. \$11.95.

Although he lived in what was demonstrably a much poorer society than did his descendants a century later, the average Englishman of 1831 drank more than five times as much liquor and at least half again as much beer. There were three times as many public houses per capita in which he might indulge his thirst. Alcohol enhanced almost every festivity, it was a component of almost every medical prescription, and it was popularly associated with physical well-being. The public house served as a coaching inn, as a center for news, entertainment, and club meetings, and as a welcome refuge from drab, ill-lit, ill-heated cottages. Troops were billeted there; laborers often received their wages there; and the taxes on the beverages sold there supplied a third of the national government's annual income. Any movement that sought to separate beer and spirits from Britain clearly had its work cut out. To bring to an end the consumption of alcohol in nineteenth-century Britain implied a veritable social revolution.

Such a social revolution failed to take place in early or mid-Victorian Britain but not for want of effort on the part of the numerous temperance associations whose origins and purposes Dr. Harrison chronicles, whose leader-

ship he analyzes, and whose affiliations and interconnections—religious, political, and social—he explores. *Drink and the Victorians* is a remarkable book in that, except for the internal history of the brewing and spirit-manufacturing industry, the author appears to have investigated every conceivable facet of his complex subject on the basis of primary source evidence while at the same time demonstrating a mastery of the secondary literature on kindred social reform movements in England and elsewhere. Yet the outcome is rather less a monument to polished historical scholarship than an immense quarry of facts and figures, of fascinating insights and troublesome paradoxes. The sheer mass of the data leaves the reader in a state of befuddlement and the task of distillation largely in his own hands.

Harrison identifies four distinct though overlapping types of temperance reform: (1) the pre-1830 advocates of free trade in beer as a counterpoise to indulgence in hard liquor; (2) the "anti-spirits" movement of the early 1830s; (3) the teetotal or "moral suasion" movement that gained strength in the 1830s and 1840s; and (4) the "prohibitionist" movement exemplified in the United Kingdom Alliance (1853), which, as a first step, sought parliamentary legislation permitting individual communities to forbid the sale of alcohol by a ratepayer plebiscite. Like many Victorian social reform movements, the temperance crusaders were stronger in the North of England than in the London area and far more closely affiliated with Non-conformity and political radicalism than with the Church of England and Toryism.

To the extent that drunkenness had declined in England by the 1870s—and available statistics do not support the case for any remarkable degree of improvement—Harrison attributes such change more to the increased supply of piped clean water and of nonalcoholic beverages, to the supplanting of the coach (and the coaching inn) by the railroad, and to the greater availability of recreational "counter-attractions" than to the efforts of temperance reformers who were often fanatical, puritanical, and hostile to "popular culture." In the words of F. R. Lees, "No people were ever yet theatred into sobriety, danced into morality, or fiddled into practical philosophy" (p. 334).

Not that the author sees Lees and his cohorts

as no more than witting or unwitting capitalist stooges diverting workingmen from their true class interests. To the contrary, the temperance movement did call attention to a genuine evil, show compassion for the victims of that evil, and foster working-class respectability and self-reliance in the face of upper-class hostility. It also forwarded political democracy, demonstrated a willingness to use state power for purposes of social reform, and promoted Victorian social stability.

Successful as he is in steering between twin pitfalls—the partisanship and antiquarianism of the temperance enthusiast on the one hand, the patronizing contempt and facetiousness of the outsider on the other—Harrison does not altogether escape conveying an occasional sense of inconsistency and amorphousness. His final chapter, a somewhat uncritical summary of the twentieth-century socialist critique of the temperance movement, does not truly fit into an account that ends somewhat arbitrarily with the Licensing Act of 1872. Yet his study more than makes up in energy and industry what it may lack in organization, and future historians of Victorian England will have no excuse for neglecting a subject whose centrality Harrison has established in such abundant detail.

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ALEXANDER LLEWELLYN. *The Decade of Reform: The 1830s*. New York: St. Martin's Press. 1972. Pp. 221. \$7.95.

There is a tide in the output of specialized monographs, which, when taken at its flood, leads on to synthesis. With all due apologies to the Bard, one might say that Alexander Llewellyn has taken the tide at its flood. Building on the research of Gash, Kitson Clark, Briggs, McCord, Best, and a host of other specialists who have turned their microscopic attention to various aspects of the Reforming Thirties, Llewellyn has produced a soundly reasoned and well-written synopsis of the period.

In the first three chapters he deals with the rise and decline of Whig liberalism, from Lord Grey through the Reform Bill to Melbourne. Then he turns to the issues of poverty, Chartism, and the origins of the campaign against the Corn Laws. The final section, perhaps the

most disappointing, is given to public opinion as expressed on foreign affairs and imperial interests on the one hand, and on matters of church and state on the other. By its very nature, such a panorama runs the risk of leaving the specialist frustrated, tantalized by the sketchiness of the analysis. Certainly one would like to have documentation on the ideas and data that the author has obviously lifted from recent works.

Yet the synthesis, good and helpful, represents a sensitive blending of ideas, men, and events. It presents the issues of reform as complex as in fact they were. With a firm grasp of the recent trends in research, Llewellyn stresses the regional as well as the class distinctions in the England of the 1830s. And to his credit, he avoids a whiggish view of that period dominated by the Whigs and subsequently frozen by Whig historians. Here are no angels (liberal Whigs) or devils (reactionary Tories), nor are we exposed to any of those retrospective suggestions that in the reforms of the 1830s can be perceived the first steps toward democracy or the origins of the welfare state, ideas far removed from Grey, Melbourne, Chadwick, Cobden, and Oastler. "A generation should be portrayed with reference to its own values," according to Llewellyn. He adheres admirably to his principle.

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Orono

NORMAN GASH. *Sir Robert Peel: The Life of Sir Robert Peel after 1830*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. xx, 743. \$22.50.

This second volume (the first appeared in 1961) completes Professor Gash's monumental biography of Sir Robert Peel. The whole provides, for the first time, an account of Peel's entire career that meets modern standards of scholarship and that reflects the recent enormous increase in our knowledge of British political history in the years of Peel's active life.

Peel played an important though ambiguous part in the development of the new structure of British politics after the First Reform Bill. His progressivism offered an acceptable alternative for liberal, moderate men who were dissatisfied with the Whigs. He also managed to appeal to leaders of the professional, mercantile,



and industrial communities and to persuade many of them that their true interests lay in maintaining the institutions of the aristocratic elite. By his middle line he was able to broaden the appeal of the Conservative party and to attract to it a wider support that enabled it to win, in 1841, a major electoral victory.

Yet, his great administration of 1841-46 proved to be a difficult one, despite its enormous initial advantages. Tensions among the Conservatives remained and were actually heightened by Peel's efforts to modernize the party by combining different elements within it, which made it more powerful but less harmonious. Also Peel, once in office, went ahead a little fast for the always apprehensive right wing of the party, and began to lose the support of sections of his former followers as early as 1844 and 1845. He showed force and determination in pushing on, even when he anticipated that his policies might result in the overthrow of his government. His force appears also in the extraordinary efficiency of his administration, an efficiency that was achieved by his constant, minute supervision of the work of every department, dealing with each cabinet member separately, at great cost to himself in effort and fatigue. Peel's difficulties with his party were increased by his strong feeling that a minister should follow his own judgment rather than party wishes and by his repeated declarations, which his actions confirmed, that he would not be the instrument for giving effect to opinions in which he did not concur. There was a contradiction for Peel, Gash writes, between his attitude as minister of the crown and his position as head of the party, that he never resolved. The situation was aggravated by his lack of adroitness in dealing with the party backbenchers: his aloofness and impatience, and his unwillingness to spend time with them explaining his policies or cultivating their good will.

Gash has an obvious partiality for his hero. He argues, and makes a good case, that Peel's thinking on major issues was original and intelligent, directed not toward palliatives but toward resolutions of basic problems. Peel had, by the end of his second ministry, an impressive list of legislative and administrative achievements to his credit. He addressed him-

self to the task of social reconciliation, and the belief was central to his policy that the interests of industry, commerce, and agriculture were not in conflict but were interdependent. In the 1840s he sought to avoid treating the Corn Law issue as a class question and to prevent the aristocracy from becoming engaged in a battle over supposed class interests in which it could only lose. Though he favored the aristocratic institutions of the country, he insisted that the aristocracy could survive only if it showed a readiness to promote the welfare of all other classes in the community. Gash deals in effective detail with the development of policy on, among other things, the Ecclesiastical Commission, the income tax, the Corn Law question, the budgets of 1842 and 1845, and Ireland. He particularly stresses Peel's intense concern with the condition-of-England question, for which his solution was not the reduction of working hours but, rather, fiscal policies through which he hoped to make England a cheaper country to live in and to increase the purchasing power of the masses. In this sense, Gash holds, Peel's apparent obsession with tariffs and finance had a strong human and social side.

The book, however, is not uncritical. Gash has reservations on small matters, such as Peel's architectural taste, and large ones, such as Peel's misjudgments on the important Bank Charter Act of 1844 or his unskillful handling of the sugar question in the same year. More generally, the picture of Peel that emerges from this close study, though enormously impressive, is in some way unattractive: a greatly talented but difficult man, and a masterful and dictatorial administrator who tended to take too much into his own hands.

Peel is a hard subject for a biography, partly because certain phases of his career were and still are controversial, and partly because his life and work bring up broad issues of British politics and can be understood only in this general context. Gash has unusual qualifications for the task. His two earlier monographs, which deal with the 1830s and 1840s, the decades covered by this volume, have become standard—required reading for everyone working in this period—and constitute an admirable background for writing the biography of this most distinguished statesman. Gash is par-



ticularly good on the larger questions, and some of the most interesting parts of the book are his discussions of these general themes. He has, in addition to his command of the primary sources, also read widely in the recent articles and monographs on the period; though he does not often cite them, those in the field will catch many points where he has taken advantage of new findings or new insights and incorporated them into his presentation. His picture of Peel is perceptive and observant, and takes us a good way into the mind of this acute but complex man. The account is rather full, but this is justified by the interest of the subject. The manner of presentation is lucid, careful, and unpretentious; Gash seems to share something of Peel's own dislike of display. The book, despite its length, is firmly constructed, and the author, as he nears the end, is able to pull his threads together effectively and to keep the perspective of the whole remarkably well. The profession is much in his debt.

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JOHN PREST. *Lord John Russell*. Columbia: University of South Carolina Press, 1972. Pp. xvi, 558. \$17.95.

Few nineteenth-century parliamentary careers were more distinguished than Lord John Russell's. For about fifty years he was a commanding public figure: principal architect of the First Reform Bill, Whig leader in the Commons from 1835 to 1855, and twice prime minister. In the great age of Liberalism few statesmen promoted Liberal causes with more confidence and enthusiasm. Yet historians have been reluctant to celebrate his achievements. Until Mr. Prest came on the scene, they had little more to show than the official life written by Spencer Walpole under the unhappy sponsorship of Russell's widow and second wife (whom Walpole called "The Influence"), together with four volumes of correspondence unsatisfactorily edited.

This neglect is not altogether unwarranted. As Mr. Prest tells us, Benjamin Jowett once observed about Russell that "no one who is so deficient in social and external qualities can ever have justice done him" (p. xv). Russell's personal defects were indeed serious, as Mr. Prest makes abundantly clear. He was shy and stiff

and proud (as only Russells could be proud). He was markedly inept in handling people, cabinet colleagues no less than his miserly brother, the seventh duke of Bedford. He was too much the doctrinaire to deal effectively with Irish troubles in the 1840s, despite his great and unusual compassion for the Irish. And he was too little the man of business to bring the sort of authority that Peel could bring to the affairs of state and society. By the 1860s when Jowett took Russell's measure, these defects had taken their toll. Professor Conacher's *The Aberdeen Coalition* has fully revealed the querulous and demeaning decline of Russell's political career in the 1850s. And Mr. Prest himself declares that when Russell's ministry fell in 1852, "he should have retired. . . . He had, after all, as H. G. Ward told him, done more in twenty-five years than was ever done before by any British statesman, and had he given up at this point posterity would have recognized this" (p. 349). Instead, "before he died Lord John had thrown away a great reputation" (p. 350).

This strikes the characteristic note of Mr. Prest's book. Although he strives valiantly to restore Russell's reputation, he acknowledges that the effort of rehabilitation—particularly in the 1850s and 1860s—is too much for him. There is perhaps some ground for complaint here. Is it fair to attribute the later fiascos of Russell's career solely to personal defects? Was it Russell's fault alone that Palmerston overwhelmed him? Elsewhere in Mr. Prest's book, for example in his account of the passing of the First Reform Bill, the wider issues of society and politics—in this case, of the sort that Professor D. C. Moore has raised in recent years—are lost in the thickets of biographical detail. Still this is not a book to grumble about. It is the product of vast research, meticulously elaborated, equally shrewd about economics as about politics, and written with wit and verve. It is full of nice touches: Russell's casual way with budgets (p. 172); the oddities of Charles Wood's behavior in cabinet meetings (p. 229); or Grey's coming to London in the crisis of December 1845 "in the sort of ill temper men reserve for the occasions on which they are about to injure their friends" (p. 206).

DAVID SPRING  
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D. A. HAMER, *Liberal Politics in the Age of Gladstone and Rosebery: A Study in Leadership and Policy*. New York: Oxford University Press. 1972. Pp. xv, 368. \$16.25.

This book might seem more original if the gist of it had not already appeared in "The Irish Question and Liberal Politics, 1886-1894," which Professor Hamer published in volume 12 of the *Historical Journal* in 1969. But it would still fall short of his claims, either for originality or for importance. For one thing, it adds little to our understanding of the reasons for the decline of the Liberal party. For another, it repeats the traditional, though unconvincing interpretation, derived from the Webbs, of the Lib-Lab alliance; it tells nothing about the relative strength or social composition of the sections whose differing priorities receive so much attention; and it says nothing about the electoral or financial structure of the party. In other words, *Liberal Politics in the Age of Gladstone and Rosebery* leaves us, despite its rather abstract tone, with the same emphasis on personalities that Hamer finds unrewarding in the work of others—the more so because Hamer tends to accept the arguments of these personalities at face value. In the case of John Morley, whose biography he published in 1968, Hamer seems to have adopted them, with certain qualifications, as his own.

Like Morley, Hamer would have us believe that the absence of any generally accepted system of thought constituted the one great weakness in the Liberal party. But other parties have been known to survive without the benefit of systems, whether Benthamite, Federalist, or Marxist, and sometimes to have survived all the better. As for the Liberals, the point, surely, is not that they were divided in purpose and principle or that the various sections competed for attention, but that they were fundamentally at odds over policy. Hamer admits that the Nonconformists, in his opinion the most "vital" of the sections, cared not at all for social reform, which was precisely what labor, the other dominant section after 1886, cared about most. Had he discussed the problem of irreconcilable interests in terms of the decline of the party, tracing it back to the eighties, if not earlier, he might then have written a very suggestive book.

Instead Hamer addresses himself to the ways

in which the Liberal leaders tried to hold the party together and, in particular, to their differences of opinion as to the relative merits of raising one issue at a time as opposed to adopting a program or, in the absence of either, of resorting to a vigorous, if negative, anti-Toryism. This last approach he associates with Harcourt, though as chancellor of the exchequer Harcourt was responsible for the most constructive reforms achieved by the Liberals from 1886 to 1906; the second Hamer associates with Joseph Chamberlain; the first with Morley, Gladstone, and Rosebery. It is they who command his attention. Stressing "form and relationship" in the determination of policy. Hamer goes on to explain Rosebery's commitment to imperialism and the commitment of Morley and Gladstone to Home Rule in instrumental terms; that is, as a means of persuading the sections to subordinate their separate interests to the achievement of one great cause, thereby creating a spirit of system and at least a temporary sense of order. This is the most convincing part of the book and perhaps the most significant, for it means we must revise our estimate of Gladstone as an impractical idealist. Yet again, the point is not that Gladstone or Rosebery or any other leader was concerned about the unity of his party but that he failed so notably in achieving it. Home Rule was the wrong issue around which to rally the Liberals. Even in 1885 it should have been clear that it was the wrong issue; and one is entitled to ask why it seemed so appealing or why, for that matter, a single issue seemed preferable to many.

Actually, the questions are related, for on the basis of *Liberal Politics in the Age of Gladstone and Rosebery*—to be more precise, on the basis of Hamer's evidence, not his commentary—it would seem that differences of opinion about party strategy had less to do with the number of issues than with their substance. Those favoring a programmatic approach favored not any but a radical program, whereas those favoring a single issue wanted one specific kind of issue: it had to be transcendent and "national" and avoid, if not shelve, the awkward problem of social reform. Home Rule, the House of Lords, disestablishment, imperialism—issues such as these were in some sense calculated to stem the pressure for reform; and for this reason one or another was considered

by each of the contenders for the leadership of the Liberal party after Gladstone retired. The differences in purpose among the contenders were not nearly so great as Hamer suggests. Indeed, stripped of the intellectualizations with which his book deals, their contests seem as personal and paralyzing as we had been led to believe.

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NANCY MAURICE, editor. *The Maurice Case: From the Papers of Major-General Sir Frederick Maurice, K.C.M.G., C.B.* With an appreciation by SIR EDWARD SPEARS. [Hamden, Conn.:] Archon Books. 1972. Pp. vi, 245. \$12.00.

The "Maurice case" is the most acute example of maladjustment in the civil-military relationship to be found in the annals of twentieth-century democratic war leadership. On May 7, 1918, in the midst of the German offensive, the London press published a letter from former director of military operations General Sir Frederick Maurice accusing the leaders of the government of "mis-statements" with respect to British troop strength and the reinforcements thereof prior to the offensive. The letter was designed to precipitate parliamentary censure; a debate did in fact take place two days later, but Asquith's motion for a committee to investigate the charges was overwhelmingly rejected. The government won; "the soldiers" lost.

The battle still rages, however, after more than fifty years. The present work is prefaced by a lengthy diatribe by the distinguished military author and man of parts General Sir Edward Spears. The significance of this "appreciation" lies primarily in its emotional tone.

General Sir Maurice's daughter Nancy has assembled a potpourri of documents on the case and its subsequent controversial historiography as a memorial and "contribution to history." Of greatest interest to the historian is the general's 1918 diary and "Story of the Crisis of May, 1918," which is dated later that month. The diary, however, is not reproduced after April 20—the crucial period in the history of the crisis! There is little to be found in either of these regarding Maurice's motivations, allegedly to save the chronically threatened Haig and maintain GHQ morale, or his relationships with other soldiers or politicians

with respect to the attempted epistolary coup d'état. The correspondence with his former chief, General Robertson, and appended diary snippets indicate Robertson's encouragement of the action and Maurice's own decision "not to see Asquith. Must take sole responsibility."

In the months and years thereafter, however, General Maurice's "pen" was at the service of several Asquithian organs, including the *Westminster Gazette*, which in 1922 as part of what might itself be regarded as an intrigue against the declining Lloyd George coalition published Maurice's *Intrigues of the War*, here reprinted. It is a general indictment of the prime minister's conduct of the war. Much of the rest of the book is devoted to the hapless Maurice's attempts to get the justice of his accusations acknowledged by Lloyd George.

This documentation will be of value to students of the Maurice controversy. They will, however, have to make their own analysis of the issues involved.

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*The Art of the Possible: The Memoirs of Lord Butler, K.G., C.H.* Boston: Gambit. 1972. Pp. x, 274. \$10.00.

Lord Butler's autobiography is too short. This is not an unmixed compliment: he writes well, he has lived in interesting times (twenty-six years in ministerial office), and he is not too bound by official restraint in what he says; but he seems to lose interest about halfway through. His boyhood in India has the charm of an impossibly dead past, his youth the promise of stepping modestly through a succession of open doors, and his first political office the interest of seeing at close quarters the fight over the 1935 Government of India Act. His account of a conversation with Litvinov in September 1938 is an interesting minor piece of evidence to help defend Britain's Munich policy—though there is no point, in a book this size, in his trying to go over the arguments about Munich.

Butler is at his best when writing about the 1940s, when he handled the 1944 Education Act and the modernization of the Conservative party in opposition. In both operations he was trying to retain what was worthwhile from the

past and at the same time to fight off churchmen—who imagined they could go on fighting each other instead of finding some way to face the growing secularism of the age—or reactionaries—who imagined that the achievements of the postwar Labour government could be reversed. His real period of authority was as chancellor of the exchequer in the early 1950s, but at this point in the book his energy seems to have run down; there are some splendid moments with Churchill, but too much of Butler's account of events is from official sources or from the *Economist*. There are some flashes of deadly candor over Suez, and then six years of silence under Macmillan. Butler is determined not to say how much he wanted to be prime minister, but he makes it quite clear how badly he and Macmillan got on. If 1951–55 is too much a matter of scissors-and-paste, 1957–61 is almost a blank. Butler's interest revives when he is describing his work in dissolving the Central African Federation in 1962–63, and he goes into considerable detail about his second disappointment for the premiership. The book is a pleasure to read, and it would be even more welcome for historians if its favorable reception led Butler to write another book about his work as Churchill's chancellor and Macmillan's home secretary.

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JEAN-PIERRE LABATUT. *Les ducs et pairs de France au XVII<sup>e</sup> siècle: Étude sociale*. (Publications de la Sorbonne, "N.S. Recherches," 1. Université de Paris—Paris-Sorbonne. Travaux du Centre de Recherches sur la Civilisation de l'Europe moderne, number 13.) Paris: Presses Universitaires de France. 1972. Pp. 456. 58 fr.

According to Labatut the great feudal peerage of the thirteenth century was transformed, largely by the creation of new dukes and peers, into a group that in the later Middle Ages became increasingly dependent on the Crown for its status and income. The assertion of what Labatut calls the state over this powerful group would never be complete, however, for newly created peers fomented rebellions and frequently pursued policies of intimidation that severely limited royal power. In the sixteenth century the Crown undertook a large-scale effort to redefine the roles and powers of

the dukes and peers, at first by drawing new creations largely from military officers of good but not particularly illustrious nobility, to act as a counter to Montmorency's and Guise's intimidation of the Crown. Labatut's thesis, then, is that it was the state, albeit with vicissitudes, that fixed the number, rank, and at least partially the income of the dukes and peers.

Labatut does well to begin his general analysis of the dukes and peers in the seventeenth century by examining whether the ideal conformed to the reality, at least for the new creations. By analyzing the surviving charters of creation and comparing their accounts of the honoring of the individual and his family through a family history drawn from other sources, Labatut proves that the remarks about military valor, fidelity to the Crown, and the antiquity of family really were true. The dukes and peers were not devalued by the Crown through creations of titles that did not conform to the ideal of military valor and personal fidelity to the king. Verifications were made to insure that the families of new creations were indeed of ancient nobility, and over seventy-five per cent of the new creations during 1589–1723 were army officers, usually of high rank. Labatut remarks that there is a striking similarity between the descriptions of chivalric behavior in charters of creation and those in the tragedies of Corneille, a fact that sheds some light on the degree of consensus about the roles and social values of this powerful elite group among the literate, wealthy, and bureaucratic elites who enthusiastically attended *Le Cid*.

The total number of dukes and peers, including the new creations from 1589 to 1723, amounted to 386, from 113 families, including ecclesiastical peers and 30 women who were titled in their own right. The average age of new creations was 48 years, indicating that this supreme honor was bestowed on persons whose military career was about over; the average age at marriage for all dukes and peers was 24½ years, and the average age at death, 59½ years. Since the list of new creations is given, it is possible to determine the number of new creations by decade, but the failure to provide the dates at which titled families die out makes it impossible to determine the absolute number of peers at any one time, or even to compare the

size of the group, say in the reign of Henry III, with its size in 1715. Clearly Labatut is interested in family history and in elite groups, but he has not pursued his investigation either to reconstitute their families or to provide career-line analysis.

Labatut's discussion of the social origins of the new creations, and of their wives, is perhaps the most important part of the book, for he discerns a pattern of new creations that shows a relationship between their families' average age of antiquity and years of social and political turmoil. Prior to 1589 ancestors of new dukes and peers on the average could be traced back to the eleventh through the thirteenth centuries, whereas for 1589–1660 the average age of antiquity of the new creations dropped to the fourteenth and fifteenth centuries. Labatut also shows that the number of new creations increased in the decades of social upheaval (though we are not told by how much), for the first time providing precise evidence about the dynamics of social change and aristocratic revolt during the Wars of Religion and the Frondes. On the average, after 1660 the antiquity of newly honored families could again be traced back to the eleventh to thirteenth centuries, which certainly suggests that Louis XIV respected the wishes of the dukes and peers regarding new creations. Only one robe official, Chancellor Pierre Séguier, was given a ducal title, significantly in the tumultuous year 1650, but even he, claiming that he lacked a son, requested that his title be bestowed on a nephew-in-law who was of gentle birth. The dominant group, then, was not tainted by robe blood. Furthermore, while Louis XIV may have favored the great ministerial families, notably the Le Telliers and Colberts, he never bestowed the pre-eminent dignity of a dukedom on one of its members.

The three principal legal ranks of dukes and peers—those with Capetian blood ranked highest, foreign princes (e.g., Turenne) came next, and those of gentle birth followed—not only established their precedence but, according to Labatut, also reflected the size of their fortune. This hypothesis is tested by a comparative study of incomes and fortunes, and on the average each legal rank did in fact represent a certain standing in fortune. Thus a hierarchy of fortune—and therefore of lands held, income,

and dignity of title—brought about an extraordinary coherence to dukes and peers, a hierarchy no doubt discernible both to royal officials and to other seventeenth-century observers.

Average incomes for dukes and peers remained steady over the century, except for the Capetian princes, whose income declined under Louis XIV. It seems correct to conclude that if the dukes and peers underwent a "crisis" in 1589–1660, neither their dignity nor their income was basically altered, and this is confirmed by the rather disconnected remarks about the *malaise striking the nobility as a whole* in the Regency of Louis XV. But here I go beyond the author's intentions. Labatut is not really interested in the broader questions of the dynamics of social change; his subject is the relationship between the dukes and peers and the state.

There is other evidence to support the conclusion that no "crisis" occurred. The frequency of intermarriage among children of dukes and peers not only remained steady, but actually increased for the sons and grandsons of newly created dukes and peers. When wives were sought for sons (the dukes and peers almost never let their own daughters marry below their rank), and a certain amount of *mésalliance* appeared inevitable, daughters were selected from robe families who were predominantly in the Council of State and therefore close to royal favor, rather than from among parlementaires. Furthermore, when this marrying down took place, it occurred most frequently within ducal families of considerable wealth and prestige. Families that were having difficulty sustaining their rank dared not marry down.

When Labatut turns to the spiritual and cultural aspects of the group he is studying, the results are fragmentary, and there is no discussion of the charitable foundations of the dukes and peers, or of their patronage of artists and scholars. Were these also steady for the entire seventeenth century? The impression, based on literary evidence, is that both declined. One senses that Labatut has all the evidence, but since these aspects of family history, or of the history of elites, do not interest him, he has left them out. The remarks about how the dukes and peers viewed their estates, their Parisian *hôtels*, and their expenditures in clothing and



furniture do help complete the picture. The fact, for example, that expenditures on luxury items were roughly the same among all the dukes and peers, regardless of their income, is brilliantly suggestive of how these families competed with one another, and how destructive this could be for families with declining income.

To conclude, it is simply not fair to criticize this work for not being an *histoire totale* of a social group. Labatut does not belong to the *Annales* school, and perhaps because of this there is an occasional lack of refinement in his statistical computations and his sense of the significance of such subjects as philanthropy and patronage. And yet in this study of the social history of an elite group Labatut has returned to one of the classic questions the *annalistes* so often side-step, whether the group is self-generating and in control of the state or whether it is dependent on the state. Labatut actually gives evidence to support both of these contentions, but instead of delineating a mechanism that delimits the dynamic both of the state and of the elite group, he simply reiterates that it is the state which is in control. True, the materials presented about the state's defense of the dukes and peers (largely a gloss on Saint-Simon) against the Parlement, for example, reflect continued support for the dukes and peers. But does not the Crown's failure to have the legitimized sons of Louis XIV fully accepted as Capetian princes score a point for the intimidating powers of the dukes and peers against the Crown?

Clearly this provocative and important study will be of great interest to social historians of France and of elite groups in general.

OREST RANUM

Johns Hopkins University

HOWARD M. SOLOMON. *Public Welfare, Science, and Propaganda in Seventeenth Century France: The Innovations of Théophraste Renaudot*. Princeton: Princeton University Press. 1972. Pp. xv, 290. \$12.00.

Although he was one of the most important protégés of Richelieu, Théophraste Renaudot (1586–1653) has not so far been studied in depth. Founder of the *Gazette*, the earliest successful French newspaper, sponsor of the *Conférences du Bureau d'Adresse*, originator of

employment offices in Paris, as well as of a low-interest loan bank, pamphleteer and popularizer of science and erudition, his free medical consultations for the poor led to his becoming the victim of a rather sordid drama involving two cardinals, the medical faculty, and the Parlement of Paris, and ending his life in misery and disillusionment. His enemies succeeded in depriving him of posterity's approval, creating a mass of legends that Dr. Solomon shows to be false in many respects. This amply documented portrait will permit the rectification of accounts of Renaudot found in many otherwise largely authoritative studies of the social history of seventeenth-century France.

Modern attitudes and interests are much in evidence here, particularly in matters of public welfare, health, medical care, adult education, the arts of communication, and the liberation of the practice and teaching of medicine from traditional routines. Dr. Solomon's findings have considerable relevance today, the more so as they offer an instance of how a powerful ruler can mold and conduct a nation's affairs in a period of foreign wars and deep internal stress, and finally leave his creatures helpless, their visions ruined, when he vanishes from the scene. Differences between our age and that of Richelieu are immense, but an account of this struggle between innovative government and established institutions in areas of public concern and the general good has its echoes in 1973.

Appendixes contain a nearly complete list of editions and translations of Renaudot's ninety or more publications, a dozen pages of illuminating extracts concerning the *Conférences* of the Bureau, a rich bibliography of primary and secondary sources, and a well-organized index. There is a thoughtful conclusion, but in spite of all these qualities one must say that the reading of proof is unworthy of the publisher and the author: there are innumerable misprints in the citations from French as well as in the main text.

HARCOURT BROWN

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SAL ALEXANDER WESTRICH. *The Ormée of Bordeaux: A Revolution during the Fronde*. (The Johns Hopkins University Studies in Historical and Political Science, 89th series [1971], num-



ber 2.) Baltimore: Johns Hopkins University Press. 1972. Pp. x, 160. \$10.00.

The Ormée of Bordeaux has long been one of the most serious gaps in the historiography of popular uprisings. The precocious radicalism of the movement was striking. After following the Parlement and then the princes into rebellion during the Fronde, this elusive mass constituency turned against all traditional authorities and began holding rallies, issuing manifestoes, and intimidating their betters. Finally three thousand men "carrying hatchets and mallets" took over the city in a sophisticated, planned insurrection and set up an autonomous government that lasted for over a year, complete with assemblies, courts, and police. Merchant and artisan judges dispensed class justice while crowds persecuted their social enemies in a sort of primitive reign of terror. Westrich is right: the Ormée, if not a "revolution," was certainly revolutionary, and he has performed a real service in giving us the first study of it in English.

Unfortunately this book is not a comprehensive treatment of the subject. In his excellent introduction Westrich notes the complexity of the rivalries among warring corporate factions in Bordeaux, but he fails to trace the evolution of their loyalties clearly, and thus it is hard to assess the overall situation. The role of the various groups is elusive. It would be fascinating to know how the princes viewed this dictatorship of shopkeepers and what sort of loyalties rallied the masses to their side. The influence of the Huguenots is also unclear, and we never learn what part the frequently mentioned *présidial* played, or why "youths" emerged in the last phase and not before.

Westrich has done more justice to the revolutionaries themselves. His data on the backgrounds of their leaders are useful. His analysis of their ideology makes clear its interesting combination of radical and traditional elements, though it is hard to determine how widespread the various strains of class struggle and republican sentiment were, and who held what opinion. It is also disappointing to learn so little of the social dynamics of the movement—how the members treated each other, their principles of organization, their method of infiltrating institutions, their reactions to adversity and success, their ceremonies. The

book is a good beginning, and sources were no doubt a problem. What stands out is the distinctiveness of this "class war," not its similarity to other contemporary disturbances, yet there is little sense of why such an anomaly occurred. We are left unsatisfied, wishing for a fuller and more comparative analysis of this significant subject.

WILLIAM H. BEIK

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MICHEL LAUNAY. *Jean-Jacques Rousseau: Écrivain politique (1712-1762)*. Cannes: C.E.L. 1971. Pp. 511.

Rousseau has continually fascinated, influenced, or enraged readers since he won the Academy of Dijon's essay contest for 1750 with a learned essay on the morally corrupting effects of learning. This self-proclaimed "man of paradox" has provided so many scholars with a seemingly inexhaustible subject of research that one has the right to ask whether a new study is worth our attention. After all, it is to be presumed that Rousseau himself—having been so critical of "that crowd of elementary authors" (*First Discourse*)—would have preferred that we read and judge his works without relying on interpretations by others.

Michel Launay's *Jean-Jacques Rousseau: Écrivain politique* is a legitimate exception to these strictures: the fruit of over a decade of scholarship and writing in collaboration with a new generation of researchers, Launay's work can truly be said to add significantly to the twentieth-century reader's appreciation and understanding of Rousseau and his times. As in previous books and articles, Launay combines a thorough knowledge of Rousseau with an indefatigable quest for relevant contemporary documents. The result is a careful study of the main themes in Rousseau's political writings, in which philosophical interpretation and historical analysis are admirably joined.

Scholars interested in Rousseau and his epoch will find Launay's first chapter ample justification for acquiring and reading the volume. Utilizing effectively a multitude of unpublished sources, as well as the scholarship of the last two centuries, Launay presents a perceptive analysis of the political conflicts between the Genevan populace and the upper classes, which provided the basis of Jean-

Jacques's "political education." Equally instructive is Launay's study of Rousseau's experiences in the years 1728-48, which are usually ignored by critics and students.

It is, however, Launay's treatment of Rousseau as a "political writer," from the *First Discourse* to the *Émile* and *Social Contract*, that will be of widest interest. To an unusual degree, Launay succeeds in placing Rousseau's works in their political as well as social context. Yet this is no mere survey of eighteenth-century politics, nor does Launay reduce Rousseau to a merely passive reflection of his times.

To be sure, not everyone will always agree with every interpretation. This reader, for example, finds that Launay's approach sometimes underestimates the extent to which Rousseau's thought is directed to the perennial problems of political philosophy, especially as posed by Plato, Aristotle, Machiavelli, Hobbes, and Locke. But such questions concern complementary interpretations rather than criticisms, and it is no small feat to have shown how closely Rousseau's formulations were linked to contemporary political issues in Geneva, France, or other eighteenth-century societies.

In every generation, there have been a few "Rousseauists" whose commentaries are the basis of continued scholarship and informed reading; to such names as Derathé, Guéhenno, Hendel, Lanson, Masson, Mornet, Schinz, Spink, Starobinski, and Vaughan must be added Michel Launay. Indeed it is to be hoped that *Jean-Jacques Rousseau: Écrivain politique* will be translated into English, since it is probably the most useful recent work for students and nonspecialists who otherwise might not follow current French scholarship on Rousseau.

ROGER D. MASTERS  
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PETER FRANCE, translator. *Diderot's Letters to Sophie Volland*. New York: Oxford University Press. 1972. Pp. 218. \$11.75.

Denis Diderot became acquainted with Sophie Volland about 1755, and their attachment continued until her death in 1784, only five months before his. During this time he wrote some 553 letters to her, of which only 189 survive. Peter France, lecturer in French at the

University of Sussex, has selected and translated 47 of these. His selections are representative; his translation is deft, idiomatic, colloquial, and accurate; and he has supplied an enlightening introduction, helpful footnotes, and a useful index.

The extant letters date from 1759 to 1774. They were first published in 1830 and have had through the years an enormous influence in enhancing the critical estimation of Diderot as a writer. For these letters to Sophie Volland are unexcelled in their revelation of a particularly interesting social milieu and of an infinitely rich, complex, and humane personality. Inhabiting these pages are most of the great names of the Enlightenment—d'Alembert, Mme d'Épinay, Falconet, Galiani, Garrick, Mme Geoffrin, Grimm, Helvétius, d'Holbach, Hume, Marmontel, Raynal, Rousseau, Sedaine, Voltaire, and many lesser lights. As the reviewer of this volume in the *Observer* put it, this correspondence is "one of the most fascinating of all documents about everyday life among the educated classes under the Old Regime . . ."; and, according to the *London Times*, "It is amazing that none of these letters has been published in English before."

Of the leading figures in the intellectual history of France in the eighteenth century Diderot is usually conceded to have outstripped Montesquieu in importance; and while he is not generally regarded as being the equal in significance of Voltaire or Rousseau, it is probable that he was more nearly representative of the whole Enlightenment than they. The letters to Sophie Volland are an incomparable means of looking into Diderot and into the eighteenth century. The edition at hand is a splendid book both for instruction and for entertainment.

ARTHUR M. WILSON  
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RUTH F. NECHELES. *The Abbé Grégoire, 1787-1831: The Odyssey of an Egalitarian*. (Contributions in Afro-American and African Studies, number 9. A Negro Universities Press Publication.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xviii, 333. \$11.00.

The principal title notwithstanding, this is not a full biography of Grégoire. Rather, it is an

account of his efforts to win legal equality for French Jews during the early 1790s, and for West Indian Negroes from 1789 to the end of his life. Mixing Enlightenment thought with a messianic Christianity, Grégoire believed in the equality of all men and sought emancipation as a first step in converting Jews and West Indians to Christianity and the other values of European civilization. He was a stubborn advocate of his views, often antagonizing potential support by extreme statements.

Grégoire usefully serves as a focus for the discussion of legislative moves toward legal equality during the revolutionary years, though he was not the main actor in most legislation dealing with the West Indies. Under Napoleon and then the Restoration, when Grégoire was far from the centers of power, he still served as an organizer for antislavery efforts. Treatment of Grégoire's career as an egalitarian also points up the diverse opposition to emancipation legislation, from anti-Semites during the early stages of the French Revolution to colonialists as slavery issues became more prominent. The general indifference of the French political public is also brought out, particularly as fear of radicalism became fashionable after the Terror. Without a special impetus, such as Grégoire's unusual religious convictions, French egalitarian sentiment had distinct limits.

These are some of the interesting themes that Professor Necheles brings out. Her narrative of events and publications is thorough and reliable, though at times the chronological framework produces rather choppy chapter divisions. The book does suffer from its ambivalent status, between biography and general account. Though the author clearly understands Grégoire, his motivations are not always explained; particularly, there is no effort to go beneath his professed principles to get at other factors that influenced him. Yet this is not a thorough treatment of the egalitarian movements. The main forces involved are not fully characterized, nor is the relevant balance of political power analyzed. One can agree with the appreciation of Grégoire's heroic advocacy and still want a more sweeping survey of the issues with which he was involved, particularly because Grégoire, consistently a loner, was more often influential than decisively in

charge. Without question, however, this book will be a valuable source for anyone interested either in Grégoire's biography or in the more general history of emancipation efforts during the revolutionary and post-revolutionary periods.

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RAGNAR SIMONSSON. *Frankrikes författningar* [The Constitutions of France]. (Skrifter utgivna av Statsvetenskapliga Föreningen i Uppsala, number 55.) Stockholm: Almqvist & Wiksell. 1971. Pp. 617. 55 S. kr.

From 1791 to the present France has had sixteen different constitutions. Simonsson, who for many years was a docent in political science at Uppsala, did not originally propose to describe all of these constitutions and intended at the outset only to describe the major changes of the recent past—the fall of the Third Republic, the Vichy Regime, the Fourth Republic, and de Gaulle's taking of power in 1958. But as he says in his preface, "It is impossible to understand the Fourth and Fifth Republics without full knowledge of the Third, and in order to understand that one must return to the Great Revolution of 1789" (p. 7). He has therefore set himself the task of writing a constitutional history of France since the Revolution, although to be sure, his emphases are different. The first third of the book covers the entire period 1791–1939. The most detailed coverage is given the period 1939–46, which takes up the second third, and the period 1946–69 is surveyed in the remaining third.

This is straightforward and old-fashioned political science of a kind not often seen today. Social and economic matters play little part here. His analysis of the Vichy Regime and of the Resistance clearly suffers from his ignorance of important questions that have been raised not only by French historiography but by German and Anglo-Saxon historians. In the most recent period he barely finds room for five pages on the student revolts and strikes in May and June 1968. An examination of the notes and bibliography reveal a sound knowledge of printed materials directly related to political matters, but provide no reference to other printed materials or to unprinted sources or, in

fact, to anything not readily available in many places. Simonsson's prejudices are carefully submerged, but it seems clear that he stands as a strong supporter of the parliamentary democracy of the Third Republic and its immediate successor. He clearly finds much that is praiseworthy in the democratic Left and, in the continuing debate over the question of a strong executive versus a strong legislature, prefers the latter. His prose is clear and convincing, but does not convey any excess of imaginative insights.

The book is very difficult to use. Not only does it lack an index, an all too common failing, but also lacks a reasonable table of contents, for the one so labeled lacks both pagination and numbering of chapters. In a book replete with personal names this is a serious drawback. At times there may be questions raised about his methodology. It is clear that his quotations from de Gaulle's television speeches in March and April 1962, for example, are drawn from volume 2 of the general's own *Memoires d'Espoir* (Paris, 1971). Given de Gaulle's rigorous sense of the elegance of the French language, one might expect that he would carefully edit his own speeches. Simonsson's book, in conclusion, is a good outline history of the constitutions of France. It is unclear, however, to what audience it will appeal. How many outside of Scandinavia will read French constitutional history in Swedish? The French summary, in fact, probably represents all that most persons interested in the subject will wish to examine carefully.

ERNST EKMAN

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JEAN-PAUL DESAIVE *et al.* *Médecins, climat et épidémies à la fin du XVIII<sup>e</sup> siècle.* (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Civilisations et sociétés, 29.) Paris: Mouton. 1972. Pp. 254. 59 fr.

The Perpetual Secretary of the Royal Society of Medicine, Félix Vicq d'Azyr, planned to publish a book like this in the 1790s. His project, the "Medical Topography of France," was to depict morbidity and mortality since 1776, epidemic disease, unusual illnesses, the abundance of water, and the influence of the environ-

ment including the weather on the "epidemic constitution." Progressive doctors all over France were keeping daily charts, urged on by Vicq d'Azyr, who insisted on faithful performance and accurate measurement (Réaumur's thermometer was just winning in France over Fahrenheit's and Celsius's and "pure" water was being defined by Lavoisier). The participating doctors would soon form a national staff for dispensing social medicine, or so Vicq d'Azyr projected in his "New Plan for the Reform of French Medicine," submitted to the health committee of the National Assembly in November 1790.

The French Revolution disrupted these plans, abolished the society, and seized its archives, thus preserving the statistical information that Jean Meyer recently "rediscovered" (as this reviewer did; see "Le droit de l'homme à la santé . . ." *Clio Medica*, 1970).

The present book promises to outrank Vicq d'Azyr's project owing to teamwork and improved methodology. But the volume under review deals only with the nation's climate and with epidemiology and medicine in Brittany alone. Its national purview is a promise, and, to keep us waiting patiently, the editor reprints four recent articles. Thus only half the book is new to the informed reader.

In 1966 Jean Meyer called for a broadly based study of the records assembled by the Royal Society of Medicine that was confusingly entitled "Investigation of the Academy of Medicine on Epidemics." Two research teams have divided the work: the climatologic experts under Le Roy Ladurie now present their findings. The study of epidemic disease is still being completed by J.-P. Peter.

In "Computerized Study of Meteorologic Data Assembled by the Correspondents of the Royal Society of Medicine, 1776-1792," the information is critically analyzed by Le Roy Ladurie and Desaive, correlated with grape and grain harvests, and with Labrousse's findings. Many tables are appended. The relationship of the weather to the French Revolution is becoming clearer. There follows a critical discussion of statistical validity of unreliable barometers and thermometers by O. Muller of the mathematics center at the Maison des Sciences de l'Homme.

J.-P. Peter's task is even more complex, for

medical data are less easily measured than the weather. Having decided to study not physicians or patients but diseases, Peter plunged into the differential study of diagnostic terminology. His computer has now learned a vocabulary encompassing the medical data of ten years for 120 localities. He hopes to offer answers to the hitherto unexplained high mortality of the years 1782–84 and to elucidate the “epidemic constitution” of pre-Revolutionary years. Volume 2 will reveal the results.

“Medical Personnel in Brittany in the Late 18th Century” is too modest a title for Jean Meyer’s essay. He places medical services in the context of agricultural, economic, and climatologic problems and provides a foretaste of the integrated studies that should eventually result. He adds that “the aim of this investigation is merely to place markers on a long and arduous path: this is to be a collective adventure” (p. 210).

The concluding article by Goubert, “The Epidemic Phenomenon in Brittany at the End of the 18th century,” analyzes why the Breton population decreased at a time when the general French population rose. The reason: epidemic disease.

The studies published here initiate a climatologic, epidemiologic, and medical manpower study of late eighteenth-century France. The researchers are evidently buoyed by the extraordinary richness of their initial archival resources. But only if provincial archives are found remunerative can a nationally valid inquiry result.

The present book whets one’s appetite for further teamwork from these *Annales* scholars.

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ALISON PATRICK. *The Men of the First French Republic: Political Alignments in the National Convention of 1792*. Baltimore: Johns Hopkins University Press, 1972. Pp. xviii, 407. \$15.00.

This work is a thought-provoking introduction to the prosopography of the National Convention. Mrs. Patrick admits at the outset that this is not a complete analysis of the *conventionnels*. Though she has read widely, her basic sources are: Kuscinski, Aulard’s *La Société des Jacobins*, the *Moniteur*, the *appels nominaux* of 1793, the printed speeches of the deputies on

Louis’s trial, and the *procès-verbaux* of the electoral assemblies of 1792. It can be noted, for example, that the *Moniteur* should be supplemented by the *Archives Parlementaires* and Aulard’s volumes by the *Journal des Débats* and the *Journal de la Montagne*. The author, however, has consciously limited her materials to workable proportions. She, therefore, concedes that her conclusions can only be tentative. Could a properly directed *équipe* exhausting all available evidence produce a definitive prosopography?

Mrs. Patrick presents voluminous statistics, some of which, according to her own admission, are based on inadequate information. At times it seems as if the compilation of statistics has become an end in itself. There are, nevertheless, many extremely valuable statistical materials.

Beginning with an analysis of the political alignments in the Convention during the Montagnard régime, the author then goes back to the *appels nominaux* of January-May 1793, finding consistency with later behavior, and then back to the elections of the deputies in 1792 by the secondary assemblies to discover correlations with the later political pattern. She finally presents a collective biography of the deputies (their political experience, ages, geographical origins, occupations, and their later careers) and attempts to relate it to political activity in the Convention. The work is somewhat marred by typographical errors, occasional inaccuracies, and a style that is at times infelicitous.

The first half of the book is largely an elaboration and slight emendation of the author’s article in the *Journal of Modern History* (“Political Divisions in the French National Convention, 1792–93,” 41 [1969]: 421–74). The controversy with M. J. Sydenham, which was continued in the journal in June 1971, is herein renewed. Mrs. Patrick reverts to a more traditional picture of the Convention’s division into three factions. She recognizes correctly that neither the Mountain nor the Gironde was a disciplined group and that each was composed of fluctuating cliques. She continues to use Sydenham’s “inner sixty” in a fashion to make them appear as a solid core of Girondins, an approach that Sydenham has maintained is a misinterpretation of his work. She is convincing in



her claim that the Mountain was largely carrying the burden of government after January 21, 1793.

Mrs. Patrick also raises important questions about various interpretations of other historians, including G. Pariset, J. M. Thompson, A. Mathiez, and Alfred Cobban (particularly his thesis on the nature of "the revolutionary bourgeoisie"). Despite the tentative character of many of her generalizations, Mrs. Patrick's work will long be used by anyone interested in further research on the parliamentary history and the prosopography of the National Convention.

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ROBERT L. HOFFMAN. *Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon*. Urbana: University of Illinois Press. 1972. Pp. xvii, 429. \$15.00.

Few social theorists have left so enigmatic a heritage as P.-J. Proudhon, and few have been put to so many political purposes. Of the several recent attempts to draw coherence from Proudhon's often confused but always exhilarating ramblings, this book is the most ambitious, for it attempts to reconstruct his general moral philosophy and to reveal its humanist core. In many respects this quest is successful. More significantly, Hoffman recreates Proudhon in such a way that this observation takes on real meaning: "Possibly the most important aspect of his legacy is not what he thought, but what he leads us into thinking for ourselves" (p. 5).

While Proudhon's anarchism, egalitarianism, and concept of justice appear in his early works, it was the disaster of 1848 that gave him his essential philosophical bearings. His critique of the revolutionary tradition since 1789 hardened his opposition to the state in any form, including democracy. More important, Proudhon's antiutopianism and his notions of the continued peevishness and irrationality of contemporary man became more firmly fixed. (Both of these help explain his brief flirtation with the Second Empire.) Finally, and central to all the rest of his thought, the experience of 1848 convinced him once and for all that the

only hope of mankind was in the urban and rural working classes because, despite their present brutishness, their very being embraced the essence of man: productive labor, the taproot of human dignity and fulfillment. The mature Proudhonian philosophy emerges thereafter. Justice, the product of "respect, spontaneously felt and reciprocally guaranteed, for human dignity," lies at the heart of Proudhon's thought. Through this concept he reconciles radical freedom with the goal of an organic society. It is the medium through which the manifold contradictions of life may be held in dynamic, unfolding balance. It is the means by which self-interested individuals contract with one another, first in the context of their natural communities, then between such groups, to generate the "collective reason," the essential moral law of a society. From it derives his well-known social ideal of mutualism. Hoffman also brings coherence to Proudhon's concept of revolution. It is the long, slow, and never-ending process of the moral regeneration of man to be implemented through the development of a rational appreciation of the individual benefits of reciprocal justice. Force, even in the name of justice, is likely to result only in other forms of domination.

Yet one cannot accept Hoffman's assessment uncritically. For one thing, the scope of Proudhon's constructive thought is rather narrow. In terms of sheer volume, his critical talents, his merciless exposure of human foibles and contemporary social and economic contradictions, dominate his writing. Frankly, it is this negative Proudhon that remains most compelling for me. But Hoffman gives less attention to this Proudhon. As a result, while he produces an erudite appreciation of the deeper wellsprings of the man's philosophy, there is a flat and often redundant quality to this book that will disappoint many readers. There are also important inconsistencies at the center of Proudhon's thought that are glossed over. It is a strange libertarian who can accept aspects of anti-Semitic mythology and take an equivocal position on the question of black slavery. More fundamentally, Proudhon's male chauvinism and patriarchal concept of the family weaken his arguments against domination in an almost irrevocable manner, especially, as Hoffman says, since Proudhon feels that "from . . . the



family experience itself [children] can acquire the basis of a moral perspective and inclination out of which justice in the larger society will evolve." Moreover, Proudhon the atheist would endorse religion as good medicine for women much as Voltaire prescribed it for the masses. There is perhaps more mindless parochialism in Proudhon than the author might care to acknowledge.

A final chapter deals with the problem of Proudhon's influence and his meaning today, underlining—as did Alan Ritter in an earlier study—the relevance of his concept of human autonomy and dignity.

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F. W. J. HEMMINGS. *Culture and Society in France, 1848–1898: Dissidents and Philistines*. (Studies in Cultural History.) New York: Charles Scribner's Sons. 1971. Pp. 280. \$12.50.

This potentially valuable study is exasperating to read. Wide-ranging erudition is frittered away by an uncertain focus and chaotic organization. To be sure, the rubric "Culture and Society" is at the bottom of the difficulty. Neither term receives a precise denotation and their relationship is ambiguous. The author recognizes the hazards of theorizing "about the causal connections between social and cultural development" and avoids making such connections, but what he provides is a grab bag of information about the milieu of the arts.

The presentation is framed by a discussion of Lamartine's part in the revolution of 1848 and of Zola's crusade in behalf of Dreyfus fifty years later. This framework implies that art is to be judged by the way in which it relates to public affairs, and that the abstention of French artists from participation in public life is somehow atypical. Within this framework the discussion is vaguely chronological and insufficiently topical. For example, a chapter entitled "The Artist as Pariah," dealing with the rise of Impressionism, is broken in the middle by an account of the events of 1870–71 and the reaction of artists and writers to those events.

The central fact in the cultural life of this period, one of which the author is clearly aware, is the conflict between convention and revolt, but he does not give this collision its

full dramatic value. Both the Second Empire and the Third Republic were suspicious of and hostile to innovation. The academies tried to suppress it by ridicule and ostracism, just as government, somewhat unpredictably, used its powers of censorship when it felt itself threatened. The culture of this age, like that of most ages, is best seen in terms of the simultaneous presence of three currents: an undertow of convention and academicism; an increasingly dominant forward thrust of innovation; and, of course, frothy eddies representing what we call popular culture running athwart and often concealing the main currents of art. The gathering momentum and ultimate triumph of these new forces in literature and painting need organized presentation, not dispersed commentary—especially in the case of drama, which is barely mentioned. Above all it needs to be stated, and restated, that the forging of the modern in France in spite of the apathy and stupidity of authority is one of the great cultural achievements of all time.

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FRANCINE AMAURY. *Histoire du plus grand quotidien de la III<sup>e</sup> République: Le Petit Parisien, 1876–1944*. Volume 1, "La Société du Petit Parisien": *Entreprise de presse, d'éditions et de messageries*; volume 2, "Le Petit Parisien": *Instrument de propagande au service du Régime*. [Paris:] Presses Universitaires de France. 1972. Pp. xii, 649; 656–1352.

In this elaborately produced, extensively documented work, Francine Amaury presents a history of *Le Petit Parisien*, the newspaper with the widest circulation in France during much of the Third Republic. Based primarily on the archives of the Société du Petit Parisien, this account of the now-defunct journal describes the economic and institutional structure of the organization in the first volume and the political role of the newspaper under the Republic in the second.

Founded in 1876 at the moment when the Republic was taking root, *Le Petit Parisien*, after a rather hesitant beginning, acquired a very large circulation by the time of the Dreyfus affair. This impressive achievement was due in part to the able management of L.-P. Piegu (1884–88) and Jean Dupuy (1888–1919).

Both publishers structured the Société so as to retain maximum control over financial and editorial policy.

Of primary importance in explaining the increase in circulation of the newspaper from 33,000 copies in 1879 to 1,453,000 in 1914, to 2,185,000 in 1932 was the publishers' ability to take advantage of existing opportunities. These included improved means of production and distribution, increased literacy and political awareness among the popular classes, and the absence of a competitive press in the provinces.

The primary purpose of *Le Petit Parisien* was to promote the interests of the Third Republic. Founded with the approval of both Thiers and Gambetta, the newspaper was supposed to become an "instrument of propaganda at the service of the regime." While the editors always insisted that the paper was obligated to inform its readers, they nevertheless undertook, by means of effective presentation of the news, "that difficult task of inspiring and guiding" those *nouvelles couches sociales* that Gambetta believed would become the mainstay of the Republic. The management soon became aware, however, that in a democratic society the reading public often influences the editorial policy of a newspaper rather than vice versa. While *Le Petit Parisien* was able to keep its readers well informed about the development of the Boulanger crisis and the Dreyfus affair, it refrained from taking a strong stand against Boulanger and in favor of Dreyfus for fear of offending its clientele. This reluctance to assert itself editorially in the interests of preserving the Republic became particularly apparent during the 1930s when the regime was foundering.

The decline of *Le Petit Parisien* occurred as the Republic itself declined, although not entirely for the same reasons. Production costs, the success of competitors, and the advent of the radio all contributed to the ultimate demise of the paper. Despite its considerable potential for influencing grass roots opinion, *Le Petit Parisien* lost sight of its original goal, and instead of forthright editorials in defense of the Third Republic, the management appeared only too eager to determine which way the wind was blowing during the tempestuous decade of the 1930s. After the collapse of the Republic, *Le Petit Parisien* tried to save itself by

advocating a policy of collaboration, an effort that assured the paper's expiration at the time of the Liberation.

This important work will be of interest not only to students of modern French history but to anyone interested in the role of the popular press in a democratic society.

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LUDWIG ZIMMERMANN. *Frankreichs Ruhrpolitik: Von Versailles bis zum Dawesplan*. Edited by WALTHER PETER FUCHS. Göttingen: Muster-schmidt. 1971. Pp. 299. DM 65.

Zimmermann had an extraordinary opportunity to investigate the archives of the Quai d'Orsay, but the result of his research is only ordinary. He had his opportunity during the 1940-44 occupation when he was assigned to provide the German Foreign Ministry with material appropriate to its purposes. Making notes for his own purposes Zimmermann wrote this study of the Ruhr action from the point of view of a peace-loving liberal. Unfortunately the commonplaces of his language, taking France from "striking while the iron was hot" (p. 13) to "losing the fruits of her Ruhr victory" (p. 272), reflect the commonplaces that dominate his thinking. He has added nothing to the generally accepted characterization of French policy as a destructive failure.

Completed in February 1945, the study remained in manuscript while Zimmermann went on to write his similarly bland *Deutsche Aussenpolitik in der Ära der Weimarer Republik* (1958). The Ruhr book, implicitly critical of German policy in the Nazi period, is necessarily critical of French policy in the twenties, and Zimmermann died before he felt he could publish it. His successor at the University of Erlangen-Nuremberg, adding references to a few other sources, prepared it competently for publication.

Zimmermann's findings provide another argument that the secrets of the archives are rarely important or truly secret. The archives make it clear enough that the French civil and military proconsuls were trying to detach the Rhineland from Germany, but this was being loudly demanded in the French Chamber. As for the man who made policy, Zimmermann

could only show that Poincaré gave his agents ambiguous support at first, and no support when their initiatives began to produce new dangers and absurdities. The author has documented what was, for the most part, well known.

Conceived narrowly as diplomatic history and essentially limited to what Zimmermann found in the files, the book gives little sense of the greater forces behind French policy. The real factors in domestic politics and international economics tend to disappear behind the sheafs of diplomatic messages and the idiosyncracies of Poincaré, Curzon, and others.

The author's failings reduce the value of the book *an und für sich* but make it otherwise useful. Lacking firm opinions, Zimmermann collected his material with a fine lack of discrimination. He had produced an undistorted review of archival sources now unavailable and given other historians their opportunity to exploit that material to more illuminating ends.

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ROBERT O. PAXTON. *Vichy France: Old Guard and New Order, 1940-1944*. New York: Alfred A. Knopf. 1972. Pp. x, 399, xix. \$10.00.

*Le gouvernement de Vichy, 1940-1942: Institutions et politiques*. (Fondation Nationale des Sciences Politiques. Travaux et recherches de science politique, 18.) Paris: Armand Colin. 1972. Pp. 372. 59 fr.

For the past thirty years, the Vichy regime has exercised an hypnotic effect on and an insidious attraction for students of modern French history. It has been terra incognita, with most of the large and even detailed questions concerning it remaining unsolved. Now, in the most impressively researched treatment to date, Robert Paxton, author of an important study of the French officer corps under Vichy, answers these outstanding questions.

Paxton examines his subject from five different points of view: (1) active collaboration with Nazi Germany from 1940 to 1942, promptly, arduously, and enthusiastically pursued by Vichy on the confident assumption that Hitler would quickly win the war; (2) the "National Revolution," for which the Vichy traditionalists, parodying the Thermidorean *incroyables* and *merveilleuses*, furnished the

tone but not the substance; (3) the collaborators themselves, spawn of prewar civil strife; (4) collaboration during the sordid last two years of the regime, now bound *durch dünn und dick* to the Reich because of its origins, inner contradictions, and aims; (5) the "legacy" of Vichy. This final chapter is a superb examination not only of the "breaks and continuities" involved—purge of the "Old Guard" traditionalists at the Liberation, perpetuation of the "New Order" professionals, technicians, and experts—but also of the two essential questions: was France better off under Pétain than a Gauleiter? was Vichy morally justifiable? Paxton's answer to both queries is an unequivocal "no!"; there are times, he concludes, when disobedience to the state is necessary to ensure the survival of the nation, and Vichy was one of those times in French history.

Paradoxically, the book's great virtue—solidly based and scrupulously handled evidence from the German documents—creates a major problem for Paxton, who with one exception was unable to gain access to the pertinent French papers, above all to the incredibly rich dossiers and *instructions* for the postwar trials. The result is that in the analysis of collaboration, the author relies chiefly upon material that tells only the German side of the story and that, as documentary evidence, often reflects only the information that the subordinate thinks his superior wants to read. Without reviving either of the two tiresome apologies for Vichy—the *double jeu* and "sword and buckler" theses—the French documents may well provide an account with more nuances, showing that much more is involved in the collaboration moves than just a frantic desire to become part of Hitler's New Order. Relying too heavily on Warner's flawed account of Laval, and unduly influenced by the revelation in the German documents that collaboration was supported, if not tried, by the entire Vichy hierarchy, Paxton unfortunately diminishes the supreme importance of Laval and Darlan: they were, after all, the two men who promised at various stages to involve France on the side of Germany in war with Britain and even the United States.

In addition, there are various defects in the prologue dealing with the military defeat, the armistice, and the overthrow of the Republic,

and also in the account of the connections between the National Revolution, the Third Republic, and fascism. The legal bases of the Gaullist claims to legitimacy are omitted. More material is needed on the cultural life of both zones, on contacts between Germans and Frenchmen, on the activities of the young French Nazi *ultras*: Jünger, Breker, even Peyrefitte's Manouche come to mind, as well as Benoist-Méchin after 1942, and certainly Lucien Rebatet. Fortunately, some topics that Paxton does not treat at sufficient length, such as the Vichy constitutional acts, the Conseil National, and the Légion Française des Combattants, are examined in detail in the second volume under review, part of the proceedings of a colloquium on the first two years of Vichy held by the prestigious Fondation Nationale des Sciences Politiques in March 1970.

To be sure, many of Paxton's points and judgments are supported by the papers and debates contained in this publication, especially in the conclusion by René Rémond. Some have been reached before in treatments by Scherer, Hoffmann, Juin, Dhers, Marin, Jaekel, Milward, Novick, Michel, Noguères *père* and *fils*, and myself. But they are brought together here with such skill and grace that such earlier general treatments as those by Farmer and Aron are completely bypassed, and such recent neo-Pétainist examinations as those by Isorni, Auphan, Raïssac, Ryan, Tournoux, Bourget, and Blond are entirely discredited: we understand *at last* what Gaullism after 1944 is all about. Paxton's book will remain the definitive work in any language on the subject until the descendants of the Vichy bureaucratic experts, whom he so brilliantly analyzes, finally open the French archives.

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EMILIO MENESES GARCÍA, editor. *Correspondencia del Conde de Tendilla*. Volume I (1508–1509). (Archivo documental español, number 31.) Madrid: Real Academia de la Historia. 1972. Pp. 836.

Until now there was little firsthand information on the social, cultural, and even the political history of Spain in the years 1508 and 1509. With the long overdue publication of Tendil-

la's correspondence from a manuscript in Madrid's Biblioteca Nacional, an invaluable eyewitness appears in the person of one of the most important contributors to making that history: Don Íñigo López de Mendoza, second count of Tendilla (later raised to marquis of Mondéjar). Tendilla ruled the former Moorish kingdom of Granada from its conquest by Ferdinand and Isabella (1492) to his death (1515) as its first Christian captain-general and *alcaide* (castellan) of the Alhambra. He had been a renowned general and negotiator in the war on Granada; he was a successful diplomat, responsible for achieving the peace between Naples and the pope in 1486 that opened the era of Spanish hegemony in Italy; he furthered the spread of the classical Renaissance in Spain through his import of Italian humanists, artists, and architects; and he was the father of the famous five Mendoza brothers, who served Charles V (among them Don Antonio, the first viceroy of Mexico, and Don Diego Hurtado, the statesman, poet, and historian), and their sister Doña Maria Pacheco of legendary Comunero glory, who defied that emperor. (Much new information on the siblings' characters and earlier years—and for the first time their correct chronological order—are found here.)

From the cumulative effect of these letters, the impression emerges of a magnetic personality larger than life. For example, if it had not been for Tendilla's not uncritical loyalty to King Ferdinand of Aragon—then barely tolerated in Castile as regent in place of the mad queen Joan, his daughter—the unification of Spain, precarious since Isabella's death, might have broken apart. With persuasion and by example Tendilla brought back into the royalist fold a number of disgruntled Castilian magnates who were on the point of reverting to the anarchy of the previous century; he also managed to keep obedient to the king the many Andalusian municipalities; and, under the persistent threat of a new Moorish invasion, he kept southern Spain safe, even though he had the difficult task of keeping pacified a conquered people of a different creed, culture, and language. Convinced that a people "cannot love [a ruler] of whom they are afraid," he constantly took care to protect the Moriscos (former Muslims who were forcibly converted), the majority of whom were loyal to him, from the

overzealous or dishonest tax collectors of the monarch he supported and from other civil servants and clerics harassing the hard-working people. The numbers of his letters in their behalf to Ferdinand and various members of what amounted to the royal cabinet are legion. To help people in grave trouble Tendilla did not hesitate to confront even the Inquisition. (We find here that, though by the peace treaty the Holy Office had not been allowed into Granada, the Catholic Kings' pledge was in fact circumvented all the time: the inquisitor-general of Córdoba acted as "Inquisitor of Granada" as well. The results, if any, of the count's pleadings do not appear in this volume.)

Reflected throughout Tendilla's letters—in his vivid, individual style—with grace and wit and studded with proverbs are the social aspect of his time, full of curiosities; the economic base; the wide range of personalities of his friends, rivals, and relatives; and his extraordinary gallantry toward women. From his splendid residence in the Moorish royal palaces (he calls Granada "a paradise with which nothing can be compared"), he comments on world events, among them the Castilian conquest of the Peñón de Velez and of Orán, the League of Cambray, and the marriage of Catherine of Aragon to Henry VIII.

Thanks are due Emilio Meneses García for devoting his Ph.D. thesis for the University of Madrid to editing the letters and the Real Academia de la Historia for making this major publication possible. Meneses's biographical introduction, discerning and extensive, covers also the forthcoming final volume 2, where the correspondence will continue to 1513. One looks forward to finding there more of this living history, and one hopes to encounter also the index, regrettably absent here.

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J. A. FABER. *Drie eeuwen Friesland: Economische en sociale ontwikkelingen van 1500 tot 1800* [Friesland over Three Centuries: Economic and Social Developments from 1500 to 1800]. In two volumes. (A.A.G. Bijdragen, 17.) Wageningen: Afdeling Agrarische Geschiedenis Landbouwhogeschool. 1972. Pp. 400; 403-760.

The province of Friesland has long enjoyed a

special attention from Dutch historians. This agrarian and seafaring region on the North Sea has been an object of study, not because of dramatic events or its impact on the rest of the world, but because of what it was, and is. Its settlement patterns, the persistence of its language, its nonfeudal political heritage, and other, more parochial, phenomena have inspired a flourishing school of local historians and provided the subject, through the years, for a long string of historical monographs of a higher quality than is usually associated with provincial history.

The latest of these is J. A. Faber's *Drie eeuwen Friesland*, an economic and social history of Friesland from 1500 to 1800. This thesis is another of the regional studies produced at the Dutch Agricultural University's department of rural history under the direction of B. H. Slicher van Bath. The author, following a pattern established in earlier studies, considers in successive chapters demographic trends, the development of occupational structure, agriculture, major nonagricultural sectors of the economy, and social developments.

Faber does not set out to weave his material into an argument more complex than that Friesland's economy was affected by the same long-term trends as the rest of Europe. Beyond the establishment of this structure—amply demonstrated from parish records, fiscal records, price and rental data, and shipping statistics—Faber organizes his book around the explication of the available archival sources. Quite inevitably, the author's attention is attracted to the relatively abundant eighteenth-century documents; the reader searching for either a narrative account of Friesian economic history or an account with strong theoretical underpinnings will be disappointed by this study, which sticks close to the documents.

If the book has shortcomings, its strengths certainly outweigh them by far, for the Friesian archives possess some remarkably useful materials to which Faber applies skillful analysis. The chapter on agriculture succeeds in establishing the severity of the "depression phase" of 1660-1750. The production of long-term trends for rental values, taxation, and drainage costs confirms the existence of a severe profit squeeze in these decades. The famous epizootics of the eighteenth century,



which literally destroyed the cattle herds on which Friesian agriculture depended, are also the object of careful statistical analysis. Faber's work would seem to deflate the legendary importance attributed to these cattle plagues; only when livestock raising was unprofitable to begin with was the loss long-lasting and economically debilitating.

When, in the last chapter, Faber turns to the province's social history—to questions of class structure, social mobility, and the composition and conduct of the upper class—he makes the most imaginative use of his sources and speaks to issues with the broadest general interest. He is able to confirm that landowning families divided themselves into supporters and opponents of Habsburg rule during the Dutch revolt along the same lines as had their ancestors in the factional strife of the fifteenth and early sixteenth centuries, which destroyed the province's autonomy and placed it within the Habsburg orbit in the first place.

Equally impressive is the analysis of the entry of new wealth into the ruling circles as the old nobility died out during the republican period. Faber shows this process to have been consistent with the growing oligarchical organization of political power, and he is able to conclude that a remarkably stable social structure with little vertical mobility characterized the late seventeenth and eighteenth centuries. It is to this that he attributes the preservation of the Friesian language in the face of the long-standing administrative, clerical, and urban use of Dutch.

When one accepts the limitations imposed by Faber's approach to his material, *Drie eeuwen Friesland* represents a major advance in the historical data available for a uniquely interesting region.

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J. WOLTRING, editor. *Bescheiden betreffende de buitenlandse politiek van Nederland* [Selected Documents on the Foreign Policy of The Netherlands]. Second Period, 1871–1898. Volume 5, 1891–1894; volume 6, 1895–1898. (Rijks geschiedkundige publicatiën, Major Series, numbers 132 and 138.) The Hague: Martinus Nijhoff. 1970; 1972. Pp. xlvii, 786; 1, 846.

The study of late nineteenth-century diplo-

matic history and the world-wide repercussions of the "new imperialism" has already been greatly assisted by the publication of the official Dutch state papers (see *AHR*, 70 [1966–67]: 134–36; 72 [1966–67]: 1409–10; 75 [1969–70]: 519). Now that the last decade of the previous century has become the focal point of the latest bulky volumes, the more limited and parochial (translate as exclusively Dutch) matters tend to be diminished somewhat and the global politics of major nations begin to invade any and all considerations. The far-reaching implications and consequences of Dutch foreign policy in these few years involve several major disputes or confrontations that American, German, British, French, Japanese, and even Russian historians should carefully scrutinize, especially in volume six. Although perfectly understandable, the use of the mother tongue in more than ninety per cent of the documents will cause some difficulty and maybe even keep some investigators away from the collection entirely. It should not do so, especially if source materials are sought on the Boer War, Anglo-French rivalry in East Africa, and both Russian and American interests in the Pacific. At least some fresh perspectives on the prelude and initial commercial and strategic steps of the Spanish-American controversy are included.

Woltring has selected primarily documents that only peripherally touch on issues beyond direct Dutch activity or involvements. In a well-done introduction, which can be read with profit by those who have only a casual interest in these years and their events, the editor carefully but briefly sketches the basic contributions of the contents in the two volumes. An enormous amount of effective work has been done, and it is, in one sense, unfair to comment about the failure to give brief biographical portraits of the main figures or cross references to additional state and related private materials, both of which would have lightened the load of future researchers. It is not inappropriate, however, to note such omissions, since many scholars of non-Dutch history requiring background on major personages and other helpful collections will consult these pages.

The entire study of the hectic era of international tension and overseas power politics of



the imperialist countries is supplemented for the first time in the last volume. This is conveyed in the growing caution of the official Dutch policies, which often stand in direct contrast to the seemingly precarious situations designed by the greater powers. The old and the new intermingled; the long and tortuous Archine War continued, reflecting the traditional and isolated Dutch colonial endeavors of the past, while the nineties introduced serious economic interests that led the Dutch (and other powers) into recurrent diplomatic crises.

These publications have been delayed too long. The impressive results so far will render valuable aid to many specialists. They also suggest that the last period from 1899 to 1919 will probably contain even more significant contributions.

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L. DE JONG. *Het Koninkrijk der Nederlanden in de tweede wereldoorlog* [The Kingdom of The Netherlands in the Second World War]. Volume 4, *Mei '40-maart '41* [May 1940-March 1941]. In two parts. (Rijksinstituut voor Oorlogsdocumentatie.) The Hague: Martinus Nijhoff. 1972. Pp. xv, 495; vii, 497-992.

The fourth volume of de Jong's "official history" begins with the establishment of the German administration after the Dutch capitulation on May 14, 1940, and ends with the protest strike of February 1941 in Amsterdam. De Jong chose this period as a unit because its main theme is the failure of the German attempt to win over large segments of the Dutch people to voluntary collaboration with the New Order that Hitler was confident of establishing during his "summer of glory." The February strike of 1941 was a clear and vigorous demonstration of the failure of this policy. For that reason it forms the logical conclusion of the present volume.

The broad outlines of German policies and Dutch responses to these policies have been reasonably well known to students of the period, but de Jong furnishes details and insights that lend richness and depth to the story: How Seyss-Inquart, the *Reichskommissar*, first sought to implement with a certain degree of suppleness and flexibility the assignment given to him by Hitler—to win over the Dutch peo-

ple to the New Order; how the Dutch, after the initial despair and numbness following invasion and defeat, began to rally in defense of traditional values such as loyalty to the House of Orange and the retention of "Dutch" traditions; how the Dutch Nazi party became the target of patriotic condemnation; and how the initial proclivity toward cooperation with the occupying power changed into rejection as a result of a number of German political actions, such as the German support of the Dutch Nazi party (NSB) and the beginning of the persecution of the Jews.

Within these broad outlines this reviewer found many new insights. The chief of these perhaps was the extent to which the Dutch secretaries-general (with only one or two exceptions) wholeheartedly cooperated with the German administration, because they unreservedly agreed with the German demands that law and order be maintained at all costs and that the country should go back to work for its own sake and, if necessary, for that of the German war effort. What is particularly striking is that the two secretaries-general concerned, in this period, even wholeheartedly supported the use of the Dutch police in investigations of resistance activities, with the result that the main resistance group of 1940 (*De Geuzen*) and the main espionage agent dispatched from London (van Hamel) were tracked down by Dutch police.

This cooperative attitude of the secretaries-general is accentuated by the contrast with the firm position taken by the Dutch commander in chief, Winkelman, immediately after the capitulation, particularly in respect to the use of Dutch industry for German military orders; it is also highlighted by the early awareness in the summer of 1940 of the need for a firmer stand on matters of principles evidenced by a number of prewar political leaders.

De Jong adds a great deal to the known picture in his portrayal of the changing positions of traditional party leaders and of the internal development of the Netherlands Union, the unity movement founded during the summer of 1940 in an attempt to thwart the claims to power of the NSB. He brings out the fact that at first Seyss-Inquart and some of his associates hoped that the Union might provide a mass basis for cooperation with Germany, until it

became abundantly clear that the Union was primarily seen by its followers as an organization meant to fight the NSB and indirectly the Germans.

In general, de Jong's description of the workings of the German administration depicts the *Reichskommissar* as a stronger figure than some previous observers have indicated, even though de Jong admits that most of Seyss-Inquart's immediate subordinates had independent bases of power in the Reich satrapies of SS, party, and government. De Jong claims that the understanding between Himmler and Seyss-Inquart was close, even though in Holland Seyss-Inquart steered a careful course between the conflicting interests of the SS and NSDAP. De Jong portrays the military commander General Christiansen even more negatively than previous descriptions, pointing out that he almost never used the potential authority inherent in his position to moderate the political course of the *Reichskommissariat* or of the police. Along this line, this reviewer noted with special interest that Bormann's representative in the *Reichskommissariat*, Schmidt, who sometimes has been portrayed as a "moderate," was in effect the chief instigator, within the German administration, of the Dutch Nazi excesses that directly led to the February strike.

One of the outstanding features of the present volume of de Jong's work is the excellence of the biographical portraits he draws, particularly those of his Dutch compatriots. General Winkelman, the secretaries-general, some of the chief political leaders, and even the German masters are described with a sensitivity and insight that, particularly in the case of the Dutch, it would be hard to excel. De Jong also continues to be able to view individuals and events with a finely balanced judgment that incorporates the world of 1940 and 1941 as it looked then to actors and victims, as well as the "wisdom of hindsight" that has to be applied with such care in dealing with Nazi policies and actions.

Perhaps there is a price to pay for all this richness and detail: at times it seems that the "broad picture" of the period does not come through quite as clearly as it might have in a shorter treatment. Perhaps the author's summary of the first seventeen chapters of the book

at the beginning of the final climactic chapter on the February strike is the result of de Jong's recognition of this problem. Be that as it may, this volume, like the preceding ones, is an invaluable aid to an understanding of this chapter of Dutch history.

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LEO TANDRUP, edited with an introduction and commentary by. *Svensk agent ved Sundet: Toldkommissær og agent i Helsingør Anders Svenssons depecher til Gustav II Adolf og Axel Oxenstierna, 1621-1626* [A Swedish Agent at the Sound: The Dispatches of the Customs' Commissar and Agent Anders Svensson at Elsinore to Gustavus II Adolphus and Axel Oxenstierna]. (Skrifter udgivet af Jysk Selskab for Historie: Number 26.) Aarhus: Universitetsforlaget. 1971. Pp. 694. 85.50 D. kr.

SIGMUND GOETZE. *Die Politik des schwedischen Reichskanzlers Axel Oxenstierna gegenüber Kaiser und Reich*. (Beiträge zur Sozial- und Wirtschaftsgeschichte, number 3.) Kiel: Kommissionsverlag Walter G. Mühlenau. 1971. Pp. xiv, 410. DM 20.

HANS LANDBERG et al. *Det kontinentala krigets ekonomi: Studier i krigsfinansiering under svensk stormaktstid* [The Economy of the War on the Continent: Studies in the Financing of War in the Period When Sweden Was a Great Power]. (Scandinavian University Books. Studia Historica Upsaliensia, 36.) Stockholm: Lärmedelsförlagen. 1971. Pp. xii, 506. 80 S. kr.

The three books refer to Sweden's involvement in the Thirty Years War. The first is a source publication, which consists of a short biography of Anders Svensson, a summary and brief evaluation of his reports, and 550 pages of his dispatches. These are factual and dry, chiefly covering news about Danish policies, military affairs, diplomacy, and occasionally internal power struggles. They include news Svensson gathered about faraway events: wars in Poland, France, the Empire, or about Spanish-English negotiations. A few deal with prices, pestilence, famine, mail service, and—surprisingly and unfortunately only too little—with economic issues such as traffic in the Sound.

The question arises whether instead of the publication of a complete text, an *Inventar* ("Calendar") of the contents of the dispatches (as given in one instance by Tandrup himself, p. 581 n. a) might not have served as well, guiding the historian interested in details to

the original manuscripts. Why print reports full of news that is better known from other sources (the siege of La Rochelle or of Breda; war in Livonia; etc.), rumors that are often wrong, events that are hardly memorable, and lengthy diplomatic formulas as found in all other ambassadorial dispatches? Not all source material is valuable, and good judgment must be exercised in selecting what should be published. The editing is, however, careful, comprehensive, and informative.

The book by Goetze begins, chronologically, where Tandrup leaves off. It offers no source material or new information but is an analysis of Swedish aims in Germany. Goetze emphasizes that not religion but the centralizing policies of the Habsburgs and their maritime plans induced Gustavus Adolphus to intervene. What was right for other countries (Spain, France, England, Sweden) was to be denied to Germany. The king's aim was to finalize Sweden's *dominium maris Baltici*, annex Pomerania, and maintain the "German liberties," i.e., internal division. Like Philip of Macedonia in Greece, Gustavus wanted to erect an *imperium Macedonicum* over Protestant Germany and, possibly, seize the imperial dignity. His plans drove him to ever new military ventures, to plundering and dispossessing Germans in order to compensate Sweden's mercenary officers and soldiers. Naked force, applied *jure belli*, led not only to the destruction of the very liberties he professed to protect but to endless suffering. Oxenstierna, who himself grabbed Mainz, tried after Gustavus's death to maintain the king's aims, at least in northern Germany, while extricating Swedish troops from south Germany. Yet even there he sought to preserve Sweden's dominating political influence and to force the south German princes to pursue the war in his interest and to assume the costs of war. Eventually his policy succeeded, insofar as Sweden retained in the Peace of Westphalia all that really mattered to her in north Germany.

Goetze's well-written and thoughtful work is a valuable correction for the conventional interpretation of Sweden's role in the Thirty Years War. What his book may lack in economic analysis is, at least for certain periods, excellently provided in the third book, which contains three essays.

The first essay, by Hans Landberg, deals with financing war preparations against Poland in 1655; the second, by Lars Ekholm, with financing Gustavus Adolphus's early campaigns in 1630–31; the third, by Roland Nordlund, with financing the war after Gustavus's death when mutiny threatened in 1633. All three show that credit was the crucial factor, notwithstanding Sweden's basic assumption that war had to feed itself. While still preparing for war, with gains only in the future, Sweden herself had, of course, to raise the means. Extra taxes in Sweden were difficult to impose, but royal estates alienated by the nobility were confiscated (*Reduktion*), income from copper and iron exports diverted for mobilization purposes, new tolls levied on shipping, and troops quartered in allied lands. Still this yielded not enough, and therefore rich noblemen, like Leijensköld, de la Gardie, Königsmarck, and other military entrepreneurs had to advance vast sums, and forced loans were imposed on merchants. In 1655 King Karl Gustaf, just as earlier King Gustavus Adolphus and Oxenstierna, personally took a hand in financial matters, whose favorable outcome depended on future victories.

All three essays, accompanied by many statistics, trace in detail the financial transactions. None of them contains complete balance sheets because surviving public accounts are incomplete, and private accounts of men like Leijensköld and Königsmarck are nonexistent. We cannot even guess what the profits (including indirect rewards) or losses were. Costs of armaments and naturalia are rarely treated in the essays. But we learn much beyond what earlier investigators (Heckscher, Redlich, and others) could tell about the activities of financial agents, their demands for securities, the techniques of the loans, the mechanics of money transfers and money drafts, and the bookkeeping methods and budget tricks. We learn where money came from (for example, Amsterdam via Hamburg) and where it went, and we see how a victorious war began to "feed itself." Yet even then, new credit was constantly needed. Confiscations did not suffice—nor did seizing of lucrative offices, appropriating salt and mining rights and other privileges, securing subsidies from France and Holland, or shifting war costs onto the shoulders of foes and allies. The

last measure especially, treated extensively in the third essay ("Krig genom ombud" [War by Proxy]), was unsatisfactory because it made Sweden dependent upon her allies and threatened her overlordship. She was driven ever deeper into war by financial as much as military and political needs, into conquering, seizing, pillaging.

Nilsson's excellent summary of the three essays would have gained even more in usefulness if yet another investigation of war finance could have been added: financing the last phase, demobilization. This task offered new, and perhaps the most complex, problems since future gains by force could no longer be expected. But the scholarly treatment by the three authors—chiefly descriptive and less analytical—adds another dimension to our understanding of Sweden's military actions, politics, and finance in the Thirty Years War.

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*Lærde brev fraa og til P. A. Munch* [Learned Letters from and to P. A. Munch]. Volume 1, 1832–1850, edited by GUSTAV INDREBØ and OLUF KOLSRUD; volume 2, 1. januar 1851–30. september 1859, edited by GUSTAV INDREBØ and OLUF KOLSRUD, completed by TRYGVE KNUDSEN and JENS ARUP SEIP; volume 3, 1. oktober 1859–7. mai 1863; Tillegg, 23. mai 1830–2. november 1862, edited by TRYGVE KNUDSEN and PER SVEAAS ANDERSEN. Oslo: H. Aschehoug & Co. (Vols. 1 and 2); Universitetsforlaget (vol. 3.). 1924; 1955; 1971. Pp. 474; 485; xi, 591.

With the publication of a third volume of "Learned Letters from and to P. A. Munch," the University of Oslo has completed a task begun as long ago as 1919. The first volume appeared in 1924 and contained 253 letters and documents dating from 1832 to the end of 1850. During this period Munch made his historical debut, brought out his early publications, developed new ideas and theories in Old Norse linguistics, and advanced to a professorship at the university. (The first letters to and from C. T. Rafn, among others, are found here.)

The second volume, covering the period from 1851 to October 1, 1859, did not appear until 1955; the task of its editors was a hard one. These were the years when Munch did his best work and had the most difficult time of his life. He felt superior in gifts and accomplish-

ments to his colleagues, especially those in administrative positions, and could not keep this hidden from them. Worse yet, as far as they were concerned, he went out and proved it. As a good historian and a good Norwegian, he made friends and enemies—about one of the former to five of the latter. But he made and kept the friendship of the crown prince, later king of the Dual Kingdom of Sweden and Norway, Charles XV John. It was partly by the aid of the prince that in the summer of 1857 he set out southward looking for historical material on Norway and the Middle Ages in foreign libraries and archives. There must have been many a sigh of relief at his going from those whom he had advised, criticized, opposed, and corrected. The latter half of this volume, which takes him through Copenhagen, Berlin, Vienna, and Venice to Rome and the Vatican Archives, is most valuable.

In Rome he made the acquaintance of Father A. Theiner, secretary of the Papal Archives. Even to Munch what happened seemed almost a miracle. Within a year he was helping Theiner with dating and cataloging; in turn he was aided in his search for new things and had permission to copy all he could find on Norway. No scholar from a land of heretics had ever before been so favored; it would be long ere one was so honored again. To this gnarled Norwegian the hand of friendship was extended and firmly grasped.

When the news got about, numerous other historians from the Protestant lands strove to profit by his success and access. Not one succeeded; even Ranke was shown the door. Munch found, copied, and sent north a tremendous mass of documents, not only to Norway but to Sweden and Denmark as well. Yet his life was a hard one. When Oslo thought of him, it deemed life in Rome easy and living there cheap. He had his family with him, so to stay-at-home bureaucrats it seemed quite natural to cut down on his stipends; eliminate his position as professor, thus saving his salary; and send him his pittance by most irregular and unusual routes and people. At times Munch seems not to have known where the next day's living was to come from, let alone the next week's. Yet in the fine frenzy of the scholar, he kept finding, copying, collating, and sending home invaluable material.

Volume three, published in 1971, ends on May 7, 1863. It contains an appendix of material, dating back to 1830, that was unknown or unavailable when the earlier volumes were edited. There is also a highly satisfactory series of indexes: one of places where the unprinted letters are found and the printed ones are available; one of persons mentioned; a registry of publications; another of places and of matters dealt with; and last of all, a list of corrections, faults in publication, and necessary additions to each volume. The editors and the university might well say *Laus Deo!* for they have done a good work well.

These volumes throw light on much of the mid-nineteenth-century work in Scandinavian history, linguistics, and folklore. There is much on publication, teaching, and travel. It is not easy to transport oneself into an age when the mail between Stockholm and Oslo only went once a week; when it took fourteen days for a letter from London to reach Oslo, and twelve for one from Paris; when a package from St. Petersburg might lie at the customhouse in Oslo for three weeks because no one happened to be going in the direction of the university. There is also much on personalities and problems, for there are things that seem to be eternal in the academic experience. Munch in his day had more than his fill of two of these: the continuous battle with the bureaucrat, whose mind never rises above petty regulations; and the infighting in the ranks of the profession, where *schadenfreude* at the mishaps of a colleague looms larger than pride in one's own accomplishments. Yet there were compensations. When he left for the south on his greatest journey, he was sent from man to man, from place to place, and found courtesy, aid, and consideration everywhere, beyond what he had known or come to expect. There, he was a prophet with honor. Munch was a great man in his own way. This publication honors the university that brought it about; it memorializes a historian who brought it honor; most of all, it honors a man who was an honor unto himself.

FRANCIS J. BOWMAN

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MAGNE SKODVIN. *Norden eller NATO? Utenviks-departementet og alliansespørsmålet, 1947-1949* [Scandinavia or NATO? The Foreign Ministry

and the Alliance Question, 1947-1949]. Oslo: Universitetsforlaget. 1971. Pp. 355.

In the mid-1960s, Norway's membership in NATO came in for a new and rising wave of public discussion in the newspapers, politically oriented journals, and argumentative books. Some of the most active participants in this discussion were clearly aiming at the separation of Norway from NATO when the North Atlantic Treaty came up for renewal in 1969. In that context the Norwegian government decided to commission Professor Magne Skodvin of the University of Oslo to prepare an account, based on official documentary materials, of how Norway's decision to join NATO was made. The manuscript was to be ready by the fall of 1968, but for various reasons it was delayed.

*Norden eller NATO?* must be regarded as the single most significant book in print about any aspect of Norway's postwar foreign policy. It deals with the most crucial issue Norway has faced since World War II, and it deals with it on the basis of every scrap of documentation concerning that issue which is on file in the Norwegian Foreign Ministry. No sources were withheld from Skodvin, and no restrictions were put on him regarding their use. On the basis of this extensive material, plus other official records and a number of documents declassified for Skodvin's use by other governments, he traces the decision-making process up to that point in mid-February 1949 when Foreign Minister Halvard Lange reached his decision to recommend that Norway join NATO. It is an immensely detailed account, yet it never loses the reader's interest or attention. Here, for the first time, the full inside story of the abortive negotiations for a Scandinavian Defense Alliance is told, as seen from Oslo. Step by step we follow Lange from Karlstad to Copenhagen, to Oslo, to London, and on to Washington. Skodvin makes it crystal clear what information and considerations led to Norway's ultimate choice.

Skodvin's assignment was confined to the decision-making process within the Norwegian Foreign Ministry. He does not go one inch beyond that. Once Lange's decision has been made, Skodvin winds up the account in three brief sentences. One can only regret that he could not have gone further. Other historians must now examine the decision-making proc-



esses within the ruling Labor party, in the government, and in Parliament during the remaining weeks until the treaty was signed in early April. It should be an exciting assignment.

H. PETER KROSBY

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JÖRG RAINER FLIGGE. *Herzog Albrecht von Preussen und der Osiandrismus, 1522-1568*. Bonn: [the author]. 1972. Pp. 1078.

Whereas Gottfried Seebass, in his study of Andreas Osiander, and Walther Hubatsch, in his biography of Duke Albert of Prussia, have recently aroused the interest of Reformation scholars in two neglected Reformation leaders, Jörg Rainer Fligge has given us a detailed account of the bitter Osiandrian controversy that involved both of these men and threatened to split Lutheranism at a critical time in its development. In his doctoral dissertation, written under the supervision of his adviser Walther Hubatsch of the University of Bonn, Fligge attempts the difficult task of evaluating Osiander's work both in Nuremberg, where he was the leading clergyman during the reformation of that city, and in Königsberg, where he went at the invitation of Duke Albert as professor, preacher, and administrator.

Using the large number of printed works by Osiander, the plethora of polemical literature of this time, and the abundant archival materials in Göttingen (the depository of the Königsberg documents), Nuremberg, and Stuttgart, as well as manuscripts in libraries and archives elsewhere, Fligge provides us with an objective, detailed account of the Osiandrian controversy, not only to the death of Osiander in 1552 but to the end of the polemics concerning it in 1567. This is not a historical account for the general reader but a chronological analysis of the theology of both Osiander and Duke Albert; of the reaction of the Königsberg theologians to Osiander's disputed doctrines of justification, the essence of God, and the presence of Christ in the believer; of the arguments of Osiander's supporters and opponents; of the attempts of Albert to restore religious unity in Prussia and the Holy Roman Empire; and of the decline of the controversy and the victory of Lutheran orthodoxy.

Fligge explains Albert's loyalty to Osiander

by referring to a friendship that dated back to Osiander's religious discussions with the former grand master of the Teutonic Knights during the Nuremberg diets of 1522-24. But Fligge also shows that both men developed their theological views during their early studies in Scholasticism, Neoplatonism, and mysticism, from which they appropriated their emphases on essence and accidents and unity under God, the supreme essence. Albert, he points out, was not a mere dilettante but a well-educated lay theologian who took seriously his responsibilities as a Christian ruler with relatively little interest in social or economic matters. He served not only as an ecclesiastical prince concerned with administrative affairs but as a paternal guide in doctrinal matters, writing theological tracts, confessions, public prayers, and hymns for his people. The book has an exhaustive bibliography, a good index, and helpful illustrations.

HAROLD J. GRIMM

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REINHOLD AUGUST DORWART. *The Prussian Welfare State before 1740*. Cambridge, Mass.: Harvard University Press. 1971. Pp. xi, 328. \$12.50.

This book is a very detailed description of the endeavors of the Brandenburg princes, especially between 1640 and 1740, in numerous fields that the author regards as concerns of "the welfare state." It ranges widely from sumptuary laws to care for the poor; from regulations for the Jews to schools, universities, and academies; from Berlin architecture and public works to sanitation, medical education, and fire protection. Professor Dorwart has taken great pains not to limit himself to his immediate sources, mostly the vast material of printed ordinances; he has broadened his approach to include a description of conditions in the different fields. The result is a compendium that furnishes a great deal of useful information, as I have experienced.

More questionable is the main thesis of the author—the uniqueness of the efforts of the Hohenzollern—and his implicit claim that he deals with the whole of the Prussian state from the sixteenth to the eighteenth centuries. In fact, this book is an analysis of policy for the electorate. Material from outside Brandenburg has been very rarely used. Moreover, it is seen exclusively from above, as if only the princes



counted and as if they could operate in a vacuum. In addition, even rather undetermined rulers like Joachim II or Frederick III are presented in a harmonizing way, as almost on par with the great elector or Frederick William I. Estates, municipalities, gilds, and law courts are mentioned only accidentally. The whole story is presented as if the final stage of enlightened absolutism had already been reached at least a century earlier. Otto Hintze once remarked that a comparison of the Magdeburg *Polizeiordnung* of 1688 with the Instruction for the General Directory of 1723 reveals in numerous details the transition from "staendische Territorialstaat" to enlightened despotism. Not a trace of the former, however, can be found in Dorwart's presentation.

Similarly, the position of the prince as *summus episcopus* and his influence in Church affairs even after the dynasty had converted to Calvinism are not mentioned. Hence the concern of the rulers between 1650 and 1740 with schools is interpreted as an attempt to establish a uniform, state educational system. It should be stated, however, that Dorwart pays special attention to the influence of Pietism on Frederick William I—which is perhaps the first time that the penetrating studies of the late Carl Hinrichs and his school have borne fruit in a presentation in English. This is not the least of the merits of this informative book.

DIETRICH GERHARD  
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ERNST BARNIKOL. *Bruno Bauer: Studien und Materialien*. (Forschungsinstitut der Friedrich-Ebert-Stiftung and the Internationaal Instituut voor Sociale Geschiedenis.) Assen: Van Gorcum & Comp. 1972. Pp. xii, 574. 98 gls.

Bruno Bauer was perhaps the most important of the Young Hegelians. All of his more prominent colleagues in that movement—Hess, Marx, Feuerbach, even Stirner—have already been the subject of full-length studies. But the treatment of Bauer has been surprisingly sparse: a few articles, sections in two general books on the Young Hegelians, and a short German doctoral thesis that is almost inaccessible. With this publication, Bruno Bauer finally gets his due: an exposition of his intellectual history that is the result of a lifetime's scholarly effort.

When Ernst Barnikol died in early 1968, he had already been working on Bauer for some forty years. He had not, however, managed to complete his work, and the present *Studien* were edited posthumously by two of his colleagues. Most of the work consists of twenty-five studies covering the entire period of Bauer's intellectual activity; this is followed by a hundred or so pages reproducing archival material exclusively from the early years focusing on Bauer's difficulties with the authorities from 1840 to 1842. The whole is done with a meticulous attention to detail and an unrivaled knowledge of the archival material.

Forty years is a long time, and it might not be everyone's choice to spend it on a fairly peripheral figure such as Bauer. But any student of nineteenth-century German cultural history must be grateful for having the material so readily accessible. For although Bauer may be peripheral, he is interestingly typical of a whole current of German thought in the nineteenth century. Bauer was the central figure in the secularizing movement of the Young Hegelians and the intellectual mentor of Marx for several years. Barnikol's studies throw light on the genesis of Marx's ideas. This was the period that originally interested Barnikol, but he continues his studies into Bauer's critical liberal comments on the 1848 revolution and further to his conservative support for Bismarck's policies. He also investigates Bauer's pro-Russian views and his general cultural pessimism that shows him to be a precursor of Nietzsche and Spengler. In his evolution from a radical thinker in the Vormärz to a conservative under Bismarck he typified so many German intellectuals. Ernst Barnikol has charted this progression admirably; his editors have done an excellent job on his manuscripts, and historians will be grateful for the resulting definitive work on this minor, but typical, figure.

DAVID MCLELLAN  
University of Kent

KURT RIEZLER. *Tagebücher, Aufsätze, Dokumente*. Edited and with an introduction by KARL DIETRICH ERDMANN. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, number 48.) Göttingen: Vandenhoeck & Ruprecht. 1972. Pp. 766. DM 142.

With the appearance of this volume, one of the

most important sources on German politics during the First World War becomes available to the historical community. In 1914 Kurt Riezler was a foreign service officer attached to the staff of Chancellor Bethmann Hollweg, whose adviser and confidant he became. Riezler kept a diary of his wartime experience but, before his death in 1955, left instructions that it should be destroyed. From that fate it was rescued by the intervention of his friends, notably Theodor Heuss, the president of the Federal Republic, and by the historians Walter Goetz and Peter Rassow, who were consulted by Heuss and recommended preservation and publication. Riezler's brother, who was appalled by the acerbity of some of the references in the diary to persons still living, resisted that advice, and it was only after his death, when Riezler's daughter, Mrs. Howard E. White, permitted K. D. Erdmann to use the diary in connection with a study of Bethmann that appeared in 1964, that any part of its contents became public. The Erdmann essay aroused so much interest among historians that there was an insistent demand for access and publication, to which Mrs. White finally yielded in 1968, authorizing Erdmann to prepare this critical annotated edition.

Since Bethmann Hollweg's own papers were destroyed in the course of the Second World War, the Riezler diary provides us with the best evidence that is available concerning the chancellor's mood at critical moments during the first world conflict: the deep pessimism concerning the spiritual state of the German people, for instance, that was one of the motivating factors in his decision to risk war in the hope of breaking out of the ring of encirclement; the mixture of horror and fascination that Russia inspired in him, which perhaps distorted his judgment in July 1914; and the strain of fatalism that deepened as the war proceeded and appears to have disarmed him in his running fight with the soldiers. Riezler's notes on his frequent conversations with Bethmann are also enlightening on such subjects as the evolution of Germany's war aims, the chancellor's views on the postwar position of Belgium and Poland, and his calculations at different times about the possibility of negotiated peace.

Finally, the diary, despite its hurried and

fragmentary style, is more effective than many more systematic accounts in recreating the atmosphere of wartime politics, in which the enormity of the issues at stake, the constantly shifting situation on the war fronts and the lack of precise knowledge concerning it at any particular moment, and the disorganization of the government and the number of competing agencies vying for position made rational decisions virtually impossible and put power in the hands of the most confident and unscrupulous party, the soldiers. It was this that defeated Bethmann, this and the German people's lack of political sophistication. "Gesamttragik des deutschen Volkes," Riezler wrote after the chancellor's fall, "the blind faith in power. Who makes the gesture gets it. Bismarck slavery. Bülow theater. Hollow facade" (p. 448). And later, in April 1918, Riezler wrote that "the *milites gloriosi* are all-powerful, supported by their successes, the unmentionable military piety of the people, the propaganda machine of the *Vaterlandspartei*, etc. . . . Civil servants and journalists wholly befogged by four years of censorship, lies, and dissimulation. The great *ruere in servitium* begins, as Bethmann used to say, after Tacitus" (pp. 459-60).

Riezler's own thinking about Germany's wartime problems and her postwar position was elaborated in 1916 in a series of articles that appeared in the *Europäische Staats- und Wirtschaftszeitung*, a selection of which Professor Erdmann has included in this volume. After Bethmann's dismissal, Riezler returned to the foreign service, first in Stockholm, where his negotiations with Bolshevik agents helped prepare for the Brest-Litovsk conference, then in Moscow, where he narrowly escaped death in the *attentat* that killed Graf Mirbach, and finally in the political department of the foreign ministry. He retired from the service in June 1919 in protest against the government's signing the Versailles Treaty.

The Stockholm and Moscow phases of Riezler's career are treated in his diary, which is supplemented by the inclusion of a number of official dispatches and memoranda and an account of the Mirbach assassination that he gave to a correspondent of the *Berliner Tageblatt*. His later activities—particularly his distinguished service as head of the *Kuratorium*

of the University of Frankfurt from 1928 to 1933 and his professorship at the New School for Social Research in New York after Hitler's accession to power—Professor Erdmann discusses in a comprehensive and satisfying biographical essay, which also includes an analysis of Riezler's principal scholarly works in the fields of political science, philosophy, and esthetics.

GORDON A. CRAIG  
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HAROLD J. GORDON, JR. *Hitler and the Beer Hall Putsch*. Princeton: Princeton University Press. 1972. Pp. xii, 666. Cloth \$19.50, paper \$8.95.

Hitler's attempt to stage a putsch in Munich in November 1923 has not lacked historians. There is, however, room for an up-to-date, comprehensive account in English that offers a detailed chronology and puts the episode into historical perspective. Professor Gordon's volume is the outcome of prodigious industry and of a combing of archives that is unmatched by any of his predecessors. It is written in the snappy, gladiatorial style familiar from his earlier works. No future historian of Bavaria or of Germany will be able to ignore it, but he may well find it hard to accept many of the author's interpretations.

Gordon's general conclusion is sound: that the putsch, far from being a milestone on a march to power, was the end of the period of latent civil war since 1918 (p. 615); that the putsch left Hitler in undisputed control of the previously fragmented Right-radical movement (p. 603); that Hitler was further strengthened because outside Bavaria the Right-radicals lost most of their freedom of action (p. 604); and that Hitler's failure restored to Bavaria stable parliamentary conditions that lasted longer than almost anywhere else in Weimar Germany (p. 547)—a fact of which Gordon might have made more.

The central part of the work, however, is a reconstruction, as painstaking as it is polemical, of the events of November 8–9 and the background to them. His sections on the composition of the Nazi party and the organization of the army, though detailed, do not add greatly to the accepted picture. This is true

even of his emphasis on Nazi recruitment from the Left and/or the working class, which is surely old news by now. The section on the ever-shifting host of "partiotic bands" is more helpful, and events bore out his verdict that the moderately conservative and monarchist bands were more numerous and more influential than the radical ones. The section on the organization of the police, its political role, and the personality of the head of the *Landespolizei*, Colonel von Seisser, renders a major service: their choice of roles was even more decisive in November than that of the army.

The section on the Bavarian state crisis, culminating in the beer hall fracas, covers over two hundred pages. As a blow-by-blow account of the maneuverings and the fighting, and in particular of the twenty-four hours of the putsch itself, it could scarcely be bettered. If its overall effect is ultimately not satisfying, that is because the narrative does not elucidate the motives and *arrière-pensées* of the admittedly devious triumvirate whom Hitler sought to push into a coup—Kahr, Lossow, and Seisser. Gordon is inclined to assume that none of the three was prepared to go along with Hitler from the moment they were "hi-jacked" at their Bürgerbräu meeting, and that they delayed showing their hand only for tactical and technical reasons. This hypothesis is now impossible to disprove, but the documentation of Deuerlein, Carsten, and Hofmann, on whom Gordon is very hard, suggests that alternative interpretations should be dismissed less cavalierly.

Gordon is able to take this line in part because he emphasizes the antagonism between the Bavarian authorities and the Nazis, playing down, though not ignoring, the overlap in aims and methods and the authorities' antagonism to Berlin and the Republic. This, in turn, rests on an evaluation of the Nazis as a rabble of hotheads who had more in common with the SDS of Columbia University than with the "blue-white" establishment. If it is true that the Nazis were not conservative because there was "not a single institution that Hitler and his circle wished to conserve" (p. 5), much of what Gordon argues follows. But if not?

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LUTZ NIETHAMMER. *Entnazifizierung in Bayern: Säuberung und Rehabilitierung unter amerikanischer Besatzung*. [Frankfurt am Main:] S. Fischer. 1972. Pp. 710. DM 68.

Denazification in the American zone of occupied Germany was a monumental failure. Anyone who still harbors doubts on this score would do well to read Lutz Niethammer's massive and authoritative monograph, a somewhat enlarged version of a dissertation completed at Heidelberg under Werner Conze in 1971. Based on the records of the U.S. military government, the denazification tribunals, and the German governments in the U.S. zone, as well as on the papers and personal testimony of many key participants in the denazification program, *Entnazifizierung in Bayern* is not only the first comprehensive study devoted exclusively to this controversial subject but also the first book that presents both the American and the German side of the story.

Proclaimed at Yalta and Potsdam as one of the principal objectives of Allied postwar policy, denazification was intended to rid the conquered Reich of all vestiges of National Socialism. Implicit in the Allied declarations was the view that all but the most nominal Nazis and supporters of the regime should be pinpointed, purged, and punished. In the final analysis, however, only the American occupation authorities, strongly influenced by political pressures in the United States, took this concept seriously enough to act in accordance with it. But, as its proponents quickly discovered, denazification was more easily proclaimed than achieved. After twelve years of Hitlerian rule, Germany was more thoroughly "nazified" than most outsiders had realized. Whether out of conviction or out of expediency, millions of Germans had joined the NSDAP; many more had become members of affiliated party organizations. The sheer magnitude of the task alone made denazification by purge problematical. Moreover, as Niethammer's representative sampling shows, the mass arrests, internments, and dismissals that characterized the early stages of the program affected mostly the small fry, while many ranking Nazis and many of those involved in actual criminal acts managed to evade the net.

And yet Niethammer argues convincingly that denazification was not necessarily doomed

from the outset. There was in fact no single, well-thought-out plan that had the support of all those concerned with the program. The lack of unanimity on goals precluded uniformity in procedures. But just as there were many points of conflict and confusion, so too there were numerous opportunities for transforming denazification into a constructive and potentially successful operation. How and why these opportunities were missed are questions that the author examines with admirable thoroughness and objectivity. One major problem, Niethammer contends, was that the Americans sought to achieve fundamental political reform without significant social change. But their chosen instrument, the "personnel purge," was at once too sweeping and too restricted to elicit the support of the indigenous political forces whose cooperation was essential to the success of the denazification program. For those on the German right, the U.S. effort was too radical; for those on the German left, it was not radical enough. The conflicts engendered by these differing views, their day-to-day effects on the denazification process and on the internal politics of Bavaria, and their long-term effects on the political and social development of postwar Germany are meticulously dealt with in this book.

In the end the more conservative forces had their way. When, in 1946, the military government transferred the denazification program to the local German authorities, it was gradually transformed from a process designed to purge and punish into one designed to rehabilitate and reinstate. To be sure, Niethammer, after studying several hundred individual cases, concludes that things got somewhat out of hand; and, therefore, the rehabilitation process went considerably further than its German proponents had initially intended. In fact, well over ninety per cent of those brought before the German tribunals were adjudged to be "followers," and thus in effect amnestied. In the final analysis, then, denazification satisfied no one—except of course the real culprits, for whom it served as a smoke screen under whose cover they not only escaped retribution but also managed to creep back into public life.

This volume is a tragic monument to a misguided and mismanaged policy; it is also a triumphant monument to historical scholar-

ship. If it is not widely accepted as the definitive account of denazification under the American occupation, I shall be surprised.

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ALAN PALMER. *Metternich*. New York: Harper and Row. 1972. Pp. vii, 405. \$12.50.

This new biography should render obsolete the score of books of the same kind and format produced since Von Srbik's famous blockbuster. The narration follows closely the thread of chronology, cleverly weaving political achievements with events of private life and glimpses of Metternich's ideology and character. The ordinary reader will be delighted with the smooth swiftness of the story and the sprinkling of witticisms, while the professional historian will be gratified to find, whenever he may be curious, footnotes referring to sources and various authorities. The latter include, in addition to the well-known classics of the ample literature on Metternich, most of the recent publications, among which the most meaty is the correspondence with Wilhelmina von Sagan, published by Maria Ullrichova in 1966; among the notable books used by the author are those by Henry Kissinger, Paul Schroeder, Enno Kraehe, Arthur Haas, Donald Emerson, Manfred Botzenhart, and David Ward. Unfortunately, Mr. Palmer seems not to have been aware of the existence of my own recent contribution, *Metternich et la France après le Congrès de Vienne* (1968-71), nor of Irby C. Nichols's *The European Pentarchy and the Congress of Verona* (1971). Therefore his account shows some weaknesses in this direction; especially when interpreting Metternich's maneuvers in the congresses of Aix-la-Chapelle, Troppau-Laibach, and Verona, not to mention a number of petty details that cannot be discussed here. This is the more to be regretted as, on the whole, the book deserves to, and will probably, remain for a long while the standard one-volume biography of the famous statesman.

G. DE BERTIER

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WOLFGANG ROSAR. *Deutsche Gemeinschaft: Seyss-Inquart und der Anschluss*. Vienna: Europa Verlag. 1971. Pp. 441.

This book by a student of Ludwig Jedlicka exemplifies the trend among historians of modern Austria to free themselves from the influence of prewar polemics. Focusing on the oft-maligned quisling of Austria, Rosar draws a portrait of the "Anschluss man," an ideological type common to Austria's middle-class leadership before 1938. Based on careful research in German and Austrian archives, Rosar's findings often concur with Seyss-Inquart's own Nuremberg testimony.

Central to Rosar's interpretation is a strong emphasis on the generational kinship between men whose Weltanschauung was shaped by World War I and its immediate aftermath. Members of the "front generation"—including Dollfuss and Schuschnigg—shared a *Grossdeutsch* patriotism and differed less over the desirability of *Anschluss* than the tempo at which it should occur. This kinship, Rosar contends, explains the relationship of trust that existed between Seyss-Inquart and the leaders of the Fatherland Front. Also important was the strategy that Seyss developed as a member of the *Deutsche Gemeinschaft* (a nationalist fraternity to which Dollfuss also belonged), based on the reconciliation of the Catholic and anti-clerical nationalist camps. The complex relationship between Seyss, the Fatherland Front, and National Socialism is carefully analyzed. Cautious by temperament, Seyss opposed violence, but he also believed that Austria could slowly move toward union with Germany while time would temper the Nazi's revolutionary *Plan*. At the time, Rosar argues, this optimism did not appear unfounded since before March 12, 1938, neither the German diplomats, the party, nor Hitler himself ever developed a coherent strategy for *Anschluss*. Particularly valuable is Rosar's account of party rivalries in Vienna and Berlin and their influence on the *Anschluss* movement. In the end Seyss-Inquart was certainly used by Göring and others, but the notion that he was merely a tool, Rosar demonstrates, oversimplifies the truth.

Unfortunately, the final chapter lacks an adequate summation, and we must be satisfied with the rather weak conclusion that, while inwardly Seyss never accepted *Gleichschaltung*,



"*reservatio mentalis* alone does not absolve responsibility for events." There is no attempt to relate Seyss's experience as an *Anschluss* leader to his later activities as an occupation authority. Moreover Rosar's list of secondary sources is thin—notably missing are Dieter Ross's *Hitler und Dollfuss* (1966) and Jürgen Gehl's *Austria, Germany, and the Anschluss* (1963). But these weaknesses do not undermine the value of a book that suggests fresh approaches to the history of interwar Austria.

HARRY R. RITTER

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KLEMENS VON KLEMPERER. *Ignaz Seipel: Christian Statesman in a Time of Crisis*. Princeton: Princeton University Press. 1972. Pp. xvi, 468. \$17.50.

Four decades after his death Ignaz Seipel has found a biographer who has grasped his significance as an Austrian and European statesman and, at the same time, recognized his personal limitations. Professor Klemens von Klemperer of Smith College, author of *Germany's New Conservatism* (1957), has mastered this complex task. He has done such a thorough job of searching for relevant material in Austrian and German governmental and Church archives and in private collections of "Seipeliana," that there will be no need for another Seipel biography for many years to come.

On the whole, this is a fine piece of scholarly writing; it might have had greater human appeal had he given more color to the chapter on Seipel's youth and his formative years in the priesthood. But a fair, well-balanced picture of him as a statesman emerges as one reads on. Klemperer's judgment on Seipel and his collaborators is usually discerning; only the author's condemnation of Felix Frank (pp. 323–24), the Austrian minister in Berlin, does not seem called for to me.

To Americans, Seipel represents a rather unfamiliar type: the statesman in a soutane. Actually, Seipel would have made a spectacular prince of the Church; he was considered repeatedly for high positions in the hierarchy, and in his last months, already very ill, he toyed with the dream of becoming archbishop of Vienna and cardinal. But for all his religious devotion and his thorough grounding in

the moral teachings of his church, he was a "Christian statesman of the school of a Richelieu," as Klemperer correctly observes (p. 267). Seipel's Socialist adversaries even called him, after the bloody riot in Vienna (July 1927), the "Prelate without mercy."

His turn to the right in those fateful days is somewhat puzzling, for he had started out as a spokesman of the liberal wing of Austrian Catholicism. Although a monarchist at heart, he had served the new democratic republic loyally as leader of the Christian Socialist party and as chancellor. Because of his able diplomacy, Austria gained in 1922 financial stability by a large loan under the auspices of the League of Nations. From then on, he was an influential figure in European power politics. He was not enthusiastic about close ties with Germany, but he did not rule out the *Anschluss* at a later date. In an authorized interview of February 1926, to which Klemperer refers (p. 305) because it aroused considerable disturbance in Paris, Seipel quoted to me Gambetta's words that one should always think of it, but never talk about it. The lasting impression he made on me in that conversation coincides with the picture drawn by Klemperer: a calm, dignified, friendly, yet somewhat remote personality, a statesman of wide horizons and with a clear perception of Austria's place in Europe.

In October 1926 Seipel returned to power, but his second chancellorship was much less successful than the first. There was a fundamental change in his attitudes. He failed to strengthen the democratic basis of the republic, largely because of his unbending hostility against the Socialists, a feeling their leader Otto Bauer reciprocated. After Seipel's sudden resignation in April 1929 he turned further and further to the right, embracing the support of the fascist *Heimwehr* and advising his German political friends to take the Nazis into the government. Clearly he had reached the end of his road. Klemperer is right when he concludes that "Seipel was not a villainous, but a tragic figure in history."

FELIX E. HIRSCH

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*La fine del potere temporale e il ricongiungimento di Roma all'Italia: Atti del XLV Congresso di storia del Risorgimento italiano*



(Roma, 21-25 settembre 1970). (Istituto per la storia del Risorgimento italiano. Biblioteca scientifica, Atti dei Congressi, volume 13.) Rome: the Istituto, 1972. Pp. xix, 677.

Under the indefatigable leadership of Professor A. M. Ghisalberti, the annual congresses of the Istituto per la Storia del Risorgimento have for some time focused on appropriate (usually centennial) anniversaries. The forty-fifth congress, held in September 1970, was a culmination of the series, held to commemorate the annexation of Rome by the Italian kingdom. The event that in 1870 disgusted Mazzini, embittered Catholics, and won excommunication for Italy's leaders was celebrated in 1970 in an atmosphere of good feeling. It opened on the Campidoglio with greetings from the president of the Republic and closed four days later with a quotation from Pope Paul—perhaps the most splendid, most truly international, and least controversial of the Risorgimento *convegni*. The much-studied Roman question, once a classic of historical dispute, has apparently lost its power to divide.

The first long paper, by Father Roger Aubert, "L'Eglise face au problème de Rome," was followed by a general discussion and then by separate papers on attitudes toward the temporal power in Great Britain (Noel Blakiston), France (Jacques Gadille), Germany (Rudolf Lill), Italy (Giacomo Martina), Latin America (Carlos M. Rama), the Slavic countries (Angelo Tamborra), and Austria (Adam Wandruska). Father Aubert's essay, balanced and suggestive, places the intransigent determination of Pius IX against the background of the very diverse traditions and perspectives within the Church, and Aubert calls for closer research into the varieties and shifts of Catholic opinion. His point is reinforced by the papers on the separate countries, for in each case national traditions of politics and political thought, specific conflicts between Church and society, and particular social tensions dominate the controversy. What now attracts historians more than the issue of papal sovereignty is the insight that controversy can provide into the social fabric it once threatened to tear. Having put the question, these Olympian surveys stop short of the answers: the pleas of Pius IX were just one more strain in the domestic life of separate nations.

A related view is developed by Alberto Acquarone in a handsomely woven and subtle essay on Italian politics and the Roman problem. Taking as his theme the concern for a moral as well as political regeneration that ran throughout the Risorgimento, Acquarone underscores every faction's intense concern with Rome and the durability of the compromise solution painfully launched in 1870. In the following session, noted experts discussed in separate papers the position on the Roman question taken by the major powers: Germany (Karl Otmar Frhr. v. Aretin), Great Britain (Max Beloff), Italy (Ennio di Nolfo), Austria (Frederick Engel-Janosi), France (Pierre Guiral), Spain (Jesús Pabón, in 150 pages), and the United States (Dragan R. Zivojinović). At the end of the conference Adam Wandruska quoted the contemporary comment by Gregorovius about the taking of Rome: an event that in other circumstances would have upset the whole world passed as a little episode in a greater drama. These informed surveys of separate countries confirm the extent to which the Franco-Prussian War and domestic strife everywhere overshadowed events in Italy.

The conference closed with papers by Fiorella Bartoccini, a preview of her sensitive study of life in papal Rome; by Vittorio Frosini on juridical aspects of the Roman question; and a concluding statement by Ghisalberti in which scholarly and rhetorical traditions of Risorgimento study are elegantly reconciled. The papers in this volume, like the ceremonial occasion that sponsored them, allow few surprises; but they will, with their broad perspectives and bibliographic notes, prove useful to any student who wishes to reopen for himself questions that many once thought could never be resolved.

RAYMOND GREW  
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S. J. WOOLF, editor, *The Rebirth of Italy, 1943-50*. (Centre for the Advanced Study of Italian Society, University of Reading.) New York: Humanities Press, 1972. Pp. vi, 264. \$9.50.

The old dictum "publish or perish" should not be taken literally by neophytes in the profession. "Publish and perish" is quite possible.

if the book is a poor one. Authors of *The Rebirth of Italy, 1943-50*, please take note.

The writing of recent history is, we all know, fraught with many dangers, but they are compounded when nine different persons express their views on nine different themes, when a period of seven years is chosen for analysis that has so many years after it that everyone knows how the conundrums were answered, and when the authors seem miffed at the way things turned out.

One saving grace of the book is that it has a central theme, a theme that is especially well stated by the editor in the concluding chapter of the volume. "The 'failure' of the Resistance was bitterly noted within months of the final liberation of Italy and soured the political activities of a significant portion of the partisan generation. There was no revolutionary transformation of Italian society, the parliamentary system seemed a mere restoration of that following the First World War, and the state remained deeply marked by fascist and pre-fascist traits, features which were accepted, even accentuated by the now dominant Catholic party—'a monarchical republic of priests . . .'" (p. 213).

In addition to having a common theme the authors appear to have a common feeling that Italy had a chance to go left after the war and that the missing of the chance was a national misfortune. The first chapter, by Professor Quazza of Milan University, explains how the Resistance failed to be a revolution, and the last chapter, by Professor Woolf, director of the Centre for the Advanced Study of Italian Society at Reading University, England, describes how the entire evolution in this period worked against the realization of left-wing goals. The other authors have their own variations on this theme, blaming the Church, big business, foreign pressure, and/or lack of political sophistication for the fact that Italy created a democratic republic.

In any cooperative endeavor of this kind, some chapters are inevitably better than others. I was favorably impressed by "The Rebirth of the Party System, 1944-48," for the author is convincing in showing how conflicting ideologies and power groups on the left split their forces and how the Christian Democratic party was able to hold its divergent elements to-

gether in such a fashion that it could become the rallying point of the moderates. On the other hand, I was particularly unimpressed by "Economic Policy in the Reconstruction, 1945-51," for it deals almost exclusively with the problem of inflation, damns the policies of Luigi Einaudi out of hand, and condemns Italy's efforts to bring its foreign trade into balance instead of "milking the golden cow," meaning getting more American aid.

In spite of all the arguments and evidence presented here, I could not help but have my opinion reinforced by what I read—that Italy created a democratic republic because that was the form of government which the majority thought would best serve its interests. Italy allied itself with the Western powers, rather than with the Eastern, not because of the intervention of the Church, the pressure of the United States, or the machinations of a small coterie of politicians, but because it believed that its security would thereby be best assured. Clearly there is a history of Italy in the postwar period that is different from that seen through red prisms.

SHEPARD B. CLOUGH  
Columbia University

JOHN O. IATRIDES. *Revolt in Athens: The Greek Communist "Second Round," 1944-1945*. With a foreword by WILLIAM HARDY MCNEILL. Princeton: Princeton University Press. 1972. Pp. xiv, 340. \$11.50.

John O. Iatrides has written an adequate summary of what has been revealed by British, American, and Greek sources since 1945 about the Allied Powers and their involvement in Greek affairs during the Second World War. Given the special place of Greece in the current reassessment of the origins of the cold war, the failure of this book to penetrate to the essential issues of the "Second Round" of the Greek civil war is particularly disappointing. The absence of a critical historiographical introduction to a highly controversial subject weakens the revisionist position suggested by the author's documentation—which he himself has been too shy to emphasize. The considerable polemic literature that has appeared during the last generation has yet to be subjected to the serious criticism it deserves. Iatrides reviews at length the complicated relations,

dominated by the question of the return of the unpopular Greek king to his throne, between the British government, the Greek government in exile in the Middle East, the Greek king in London, and, occasionally, the Communist-dominated resistance (EAM-ELAS), which by 1944 was in virtual control of Greece. A notable contribution of the book is its examination of the peripheral role of the United States and especially the inclusion of the perceptive and often prophetic reports of its ambassador, Lincoln MacVeagh, to the exiled government.

Like the sources he heavily relies on, Iatrides fails to give adequate notice to the character of the EAM-ELAS coalition and, in general, to the political situation in Greece. While the exiled politicians were consumed with Churchill's firm resolve to return the king to his throne, thereby assuring Greece's place within the British sphere, the EAM-ELAS managed to establish a remarkably effective working government under the Axis occupation. The author's objective was to examine the causes of the battle of Athens, which had been precipitated, soon after liberation and the landing of British troops, by a violent confrontation between unarmed civilians and the Greek police. The immediate issue had been the British demand that the EAM-ELAS forces unilaterally disarm. Iatrides's conclusions are that both sides were unsure of each other's position and their own immediate objectives and that all sides found themselves in a bloody struggle no one wanted. This position misses the point and, furthermore, contradicts the evidence Iatrides has cited at length. Churchill, it is clearly shown, never hesitated for a moment in pursuing, despite "all costs," his objective of a liberated Greece under British auspices. On the other side, Iatrides introduces a document, the one new piece of evidence in the study, that helps to confirm what has been suspected all along: that the Communists never intended to take power by force and that they were in fact committed to a "strategy of legalism." The substance of this captured document should have been the subject of this book. The key to understanding the Greek civil war cannot be found in London, Cairo, or Washington alone, but in Athens and the Greek countryside. Both the EAM and the Greek Communist party it-

self were amalgams of political, social, and regional groups that tried in vain to reach an accommodation with the British and later the Americans; their story has yet to be told.

GEORGE FRANGOS  
Vassar College

ȘERBAN PAPACOSTEA. *Oltenia sub stăpînirea Austriacă (1718-1739)* [Oltenia under Austrian Rule (1718-1739)]. (Academia de Științe Sociale și Politice a Republicii Socialiste România, Institutul de Istorie "N. Iorga." Biblioteca Istorică, number 23.) Bucharest: Editura Academiei Republicii Socialiste România. 1971. Pp. 342. Lei 22.50.

This monograph deals with the history of that portion of Romania's territory which was separated from Wallachia by the Austro-Turkish treaty of Passarowitz (1718) and incorporated within the Austrian Empire until 1739. The province, Oltenia or Little Wallachia, was handed back to Wallachia by the treaty of Belgrade.

The abundance and exceptional value of the documentation concerning Oltenia left by the Habsburg authorities has been in part exploited by Romanian historians for several decades. However, a great deal of the material hitherto unresearched was investigated and analyzed by the author. Added to previous studies, the present investigations have allowed him to describe the whole period under its complex, ample, and unitary aspects. Șerban Papacostea pays particular attention to the population problem, and implicitly to Austrian demographic policy; interprets the economic life of Oltenia, pointing out the dominant agrarian character of the region; analyzes the status of the classes and the categories within each class; and presents the fiscal structure, the administration, and the judiciary with rich supporting documentation. Linking all these he stresses the confrontation between the efforts of the Viennese court to subordinate the province and the struggle of the Oltenian boyars to save their autonomy and preserve their power. He also demonstrates the forms of the class struggle of the peasantry, and its consequences. The peasantry, caught between two exploiting tendencies, had recourse to mass immigration, thereby temporarily succeeding in upsetting the network of the fiscal and social exploiting institutions. This compelled the

Austrian authorities to a constant re-adaptation of governmental solutions.

The analysis of the centralizing system imposed on Oltenia by the Austrians, a system very different from the traditional boyar state prevailing in the Romanian principalities at the time, opens up new perspectives on terms of historical research, for example, a new interpretation of a series of reforms introduced by the Phanariot princes of Moldavia and Wallachia after the re-incorporation of the Oltenian territory. Papacostea has completed a historical monograph, in its full sense, that focuses the attention of foreign scholars as much on the author as on the problem treated.

CORNELIA BODEA  
Boston College

N. M. LEBEDEV. *Pestel': Ideolog i rukovoditel' dekabristov* [Pestel': The Ideologue and Leader of the Decembrists]. Moscow: Izdatel'stvo "Mysl". 1972. Pp. 341.

It is reasonable to assume that any writer who undertakes to publish a new study of P. I. Pestel would be mainly motivated either by the uncovering of new sources or by an opportunity to arrive at original conclusions. Lebedev offers neither one nor the other. The author presents a true illustration of firmly established Soviet semantics with all its amorphous terminology. The work is another example of the common literary pattern with its standardized censure, approvals, and deductions so often found in Soviet historical literature.

Much has been written on Pestel, from A. I. Herzen and G. V. Plekhanov to more recent writers such as N. M. Druzhinin or B. E. Syroechkovsky. To this imposing list of publications N. M. Lebedev adds another biography of the "Decembrist ideologue." It is a lengthy monologue written with the confidence of a "party-liner" and overlaid with carefully chosen references, beginning with the epigrammatic quotation from Marx and followed by frequent citations from Lenin. The narrative is strewn with colloquialisms and hollow phrases that add little to the illumination of the secretive, intriguing personality of Pestel. The writings of Pestel or his testimony cast insufficient light either on his leadership or on his role as the ideologue of revolution. The effusive gusto

of Lebedev's style lacks methodical analysis, is void of analytical depth, exposes partiality, and frequently needs supporting evidence. A single instance may suffice. The author regards the appointment of M. M. Speransky to the investigation commission of the Decembrists as a "personal tragedy" for the aged statesman. The emperor, presumably, named Speransky to test his monarchical loyalty. This has been said by other writers; Lebedev repeats this version, yet it still lacks specific proof.

All in all, Pestel still remains the complex enigmatic personality he always has been. He is often referred to as the Russian Robespierre, as the pioneering advocate of a state dictatorship, and even as the earliest ideologue of the Soviet system. All this may be speculated upon, but the pedestrian, day-to-day account offered here presents little convincing proof. In the end, Pestel still stands as the enigmatic, rebellious aristocrat that he has been for nearly a century and a half in historical literature.

ANATOLE G. MAZOUR  
Stanford University

H. MONTGOMERY HYDE. *Stalin: The History of a Dictator*. New York: Farrar, Straus and Giroux. 1972. Pp. xv, 12-679. \$12.95.

Using a technique that has served him well in writing biographical studies of literary figures (for example, Henry James and Oscar Wilde), Mr. Hyde has undertaken the more formidable task of writing a biography of Stalin in accordance with the principle *se non e vero, e ben trovato*. If a story or anecdote appeals to him he uses it, even though it may come from a source known to be a forgery (the pseudo-Litvinov *Notes for a Journal*) or a work of dubious authenticity (the so-called Khrushchev memoirs, against which, as Mr. Hyde disarmingly informs us, Robert Conquest warned him). A number of colorful but improbable details have been gleaned from a biography of Stalin written by Yves Delbar (1953), which in turn was based in part on materials supplied by Gregory Bessedovsky, the slippery former Soviet minor diplomat whom Bertram D. Wolfe identified as the principal concocter of the Litvinov forgery.

Diligent as well as indiscriminating, Mr. Hyde on occasion has turned up interesting,

unhackneyed material, notably in the field of Soviet foreign policy during the thirties and the Second World War, periods for which British diplomats and memoirists have contributed some vivid firsthand observations of Stalin. Unfortunately, Mr. Hyde makes little attempt to use these nuggets as the basis for a coherent analysis of Stalin's character or to relate them causally to his career.

The book is worth reading for the occasional quotation one may have missed and for its engaging narrative flow, but it adds little to our knowledge or understanding of Stalin and Stalinism. In analytical depth it lags far behind the old but still unsurpassed biography of Stalin by Boris Souvarine (1939), which has recently been reprinted (1972), though without the extensive bibliographical notes that constitute one of the most valuable features of the original French edition (1935).

ROBERT M. SLUSSER

Michigan State University

A. A. GROMYKO *et al.*, editors. *Dokumenty vneshnei politiki SSSR* [Documents on the Foreign Policy of the USSR]. Volume 16, 1 ianvaria–31 dekabria 1933 g. [January 1–December 31, 1933]; volume 17, 1 ianvaria–31 dekabria 1934 g. [January 1–December 31, 1934]. (Ministerstvo Inostrannykh Del SSSR.) Moscow: Izdatel'stvo Politicheskoi Literatury, 1970; 1971. Pp. 919; 878.

Since publication of the series began in 1957, the *Documents on the Foreign Policy of the USSR (DFP)* has established itself as an essential source for the study of Soviet foreign relations and the history of diplomacy between the world wars. Volumes 16 and 17 continue the valuable contribution made by their predecessors.

Together the volumes present 891 documents (plus statistics on Soviet foreign trade in 1933) in a convenient chronological format. The value of these materials, more than four-fifths of which were previously unpublished, is amplified by extensive notes and by the inclusion of 116 related documents of foreign origin. Lists of the Soviet and foreign documents and indexes by topic and country also facilitate the reader's use of the materials.

Volumes 16 and 17 follow the pattern set for the *DFP* of focusing on the conduct of Soviet diplomacy by the Commissariat of Foreign Af-

fairs (Narkomindel). Fifty-two per cent of the Soviet documents are reports to the Narkomindel from representatives abroad or memoranda of meetings with foreign diplomats. One-fifth are dispatches from the Narkomindel to its representatives. Communications to other governments constitute twelve per cent of the total; public policy statements, eight per cent; and international agreements, seven per cent. Half of the remaining six documents are reports by the Narkomindel to the Central Committee of the Communist party or to the Council of People's Commissars (Sovnarkom). There are, however, no policy directives by the Sovnarkom or the Communist party and no suggestions of dissent within these bodies about foreign policy. The problems of Soviet historiography are further reflected by the omission of Karl Radek's many important articles. There are also no materials bearing on the Communist International.

Despite these limitations, the two latest volumes of the *DVP* add measurably to our knowledge of Soviet foreign affairs during 1933 and 1934. The development of formal relations with the United States is the subject of 75 documents. Of particular note are several, but apparently not all of M. M. Litvinov's reports about his negotiations with Franklin Roosevelt in November 1933. The *DVP* also provides the fullest Soviet publication to date of the Roosevelt-Litvinov agreements, although Litvinov's explanation of Soviet economic espionage laws is not included. The tangled and dangerous Far Eastern situation is well covered with more than 130 documents, which permit a more detailed understanding of the Soviet response to Japanese aggression. Historians of German-Soviet relations will benefit from extensive archival materials, including two economic agreements and reports of numerous meetings with German political leaders. Nearly one-fifth of the volumes are assigned to relations with France and Britain, but there are some strange omissions: for example, the temporary commercial treaties of 1934 with these two states. Still more curious, 105 documents concern relations with the four Scandinavian countries, nearly as many as pertain to the League of Nations, the Disarmament and World Economic Conferences, and Poland, Latvia, Lithuania, and Estonia combined.



Its shortcomings notwithstanding, the *DVP* is a major source for historians of Soviet foreign policy. To ignore it would be only less rash than to neglect other sources.

ROBERT HIMMER

West Virginia State College

### NEAR EAST

S. M. STERN *et al.*, editors. *Islamic Philosophy and the Classical Tradition: Essays Presented by His Friends and Pupils to Richard Walzer on His Seventieth Birthday*. Columbia: University of South Carolina Press. 1973. Pp. viii, 549. \$25.00.

It has been a long time in coming, and it comes, alas, expensively, but this *Festschrift* is a rich and rewarding collection. Richard Walzer, a classicist trained by U. von Wilamowitz and Werner Jaeger and an Arabist from the school of G. Bergsträsser, has been the most knowing student of the Greco-Arabic translation literature in philosophy over the last thirty years; here he is celebrated by thirty-three colleagues and students, some of them classicists, some Arabists, and some philosophers. The articles on Greek philosophy by Ackrill, van den Bergh, Charles Kahn, the Kneales, Theiler, Wehrli *et al.* will probably receive notice elsewhere, and so I shall here remark only on the Arabists' contributions.

First there is an important article by Franz Rosenthal (pp. 337-49) wherein he has collected, translated, and briefly studied all the references in the Arabic translation literature to the enigmatic Aristotelian commentator called in Arabic *'llynws*. Rosenthal has had no more success in identifying him than others (Aelianus? Albinus? Elias? Apollonius?), but now all the evidence is exposed in a convenient and useful form. In a somewhat parallel effort, A. A. Ghorab (pp. 77-88) has extracted and translated the citations of Aristotelian commentaries, some of them lost in Greek, by Alexander of Aphrodisias, Themistius, and Porphyry to be found in al-'Amiri's (d. 992) important ethical work *al-Sa'ādah wa al-Is'ād*.

J. N. Mattock gives us (pp. 235-59) the first full translation, from Kraus's edition, of the Arabic epitome of Galen's *On Traits of Character* (*Peri êthôn*), a work that Walzer himself has studied in the past. Georges Vajda has like-

wise translated (pp. 473-89) the chapter on the vision of God from the *Kitāb al-Muhtawī* of the Karaite theologian Yūsuf al-Baṣīr and shows its dependence on parallel passages in the contemporary Mu'tazilite theologian 'Abd al-Jabbār (d. 1025), a dependence that extends far beyond this particular chapter.

Two articles are devoted, appropriately, to Farabi. Martin Plessner has edited (pp. 307-14) the newly discovered preface to Farabi's *risālah* on medicine from Istanbul (MS Emanet Hazinesi 1730), together with the section of Maimonides' Arabic commentary on the first aphorism of Hippocrates (MS Hunt 427), where the Jewish physician cites Farabi textually. F. W. Zimmermann's essay is a detailed study (pp. 517-46) of Farabi's enumeration of opposed propositions in his commentary on the *De Interpretatione* (Kutsch-Marrow, pp. 64-65), where Farabi's "amazing collection of oddities and obscurities" is viewed against the ambiguities of the Syro-Arabic translations of that passage in Aristotle.

George Hourani and Alfred Ivry here offer summary prolegomena to their larger studies of the ethics of 'Abd al-Jabbār and the metaphysics of al-Kindī. Hourani (pp. 105-16) is sound and careful as always, and Ivry's contribution (pp. 117-40) is the best short study I know of Kindi the philosopher. H. V. B. Brown has attempted (pp. 35-48) further to disentangle Ibn Sīnā's quarrel with the *Maghribiyyūn*—here, with Pines, the Christian Peripatetics of Baghdad—by studying their respective views on the function of nature as a creative agent, consciously as in Alexander and the Baghdadis, or unconsciously with Rāzī and Ibn Sīnā.

Susanne Diwald writes (pp. 49-62) on the complex doctrine of the internal senses elaborated by the Brethren of Purity, with some interesting remarks on their connection with Manichean theories on the subject. There is a substantial and illuminating treatment of the Imamite Shi'ite attitude toward the 'Uthmanic recension of the *Qur'an* by A. Kohlberg (pp. 209-24), who finds in the Shi'ite sources a far more nuanced—and ambiguous—position than the Sunni polemicists would have us believe.

M. Schwarz takes up (pp. 437-66) the notion of "acquisition" (*kashb*), which played such a central role in the debates on ethical determination and free will in Islam, and traces its ev-



olution in literary and theological texts from the pre-Islamic "performance of an action when a judgment of value is involved," thence to the action itself, and finally to classical *kalām's* understanding of *kasb* as the relation between the agent and the action. The late S. M. Stern has assembled and instructively commented upon (pp. 437-66) the fragments of a chronological work by the ninth-century historian Abū'Isā ibn al-Munajjim. Our loss is underscored once again by this article and by the promise of what was to come (compare pp. 220, 447) from this gifted scholar.

There is more besides, even in Islamics: Louis Gardet's intelligent meditation on Mu'tazilitism (pp. 63-76); Albert Hourani's study of the modern founder of the Naqshabandi order, Shaykh Khālid (d. 1827) (pp. 89-104); the analysis by Angelika Kleinknecht, who also prepared the Walzer bibliography (pp. 5-16), of the curious exercise in Qur'anic logic known as *al-Qistās al-Mustaqīm* (pp. 159-88); and M. C. Lyons's brief, and not entirely happy, excursion into comparative psychology (pp. 225-34).

Taken all in all, which likely only the recipient and reviewer does, this is an impressive collection and one that enhances, by the quality of its contributions and the care in its making, the scholar whom it seeks to honor. *Ad multos annos!*

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D. M. DUNLOP. *Arab Civilization to A.D. 1500*. (Arab Background Series.) [London:] Longman; [Beirut:] Librairie du Liban; distrib. by Praeger Publishers, New York. 1971. Pp. x, 368. \$15.00.

Due to its multilateral nature, *Arab Civilization* may be viewed from many sides and by various scholars. Anthropologists, sociologists, philosophers, artists, political scientists, as well as historians, have the right to undertake research dealing with this topic. Needless to say, each scholar would look at it from the point of view of his own specialization. D. M. Dunlop is a well-known historian of the Middle East and possesses a good knowledge of the original classical Arabic sources—a commendable achievement for a non-Arab scholar specializing in Islamic history. In this book, therefore, he discusses certain aspects of Arab civilization

as a historian, albeit a historian with a good taste for other cultural features such as literature, philosophy, geography, science, medicine, and some famous women in Islam. As a matter of fact, these are the main topics that Professor Dunlop discusses in this book besides the two main topics dealing directly with Islamic history, namely, "The Arabs and the Arab World to 1500" (chapter 1) and "History and Historians" (chapter 3). As expected, these two chapters (1 and 3), in which the author as a historian deals directly with his discipline, represent the best and most informative portions of the book. This does not mean that the other chapters are not informative. On the contrary, as the author himself mentions in the foreword, throughout the whole book there is always "something fresh to say" (p. ix).

This book is not written for popular reading; nonetheless it is stimulating and challenging to scholars and advanced students acquainted with the problems of research in Islamic history and civilization. A glance at the references cited in the footnotes is enough to convince the reader that an all-out effort has been made in this book to examine the literary sources of classical Islam as thoroughly and as exhaustively as possible. As is often the case in such endeavors, however, all features of Arab civilization cannot be included in a single volume or by any one scholar. Accordingly, it would be unfair to criticize this work for what has been left out or minimized. Suffice it to say that what the author set out to do was done well and deserves the respect and admiration of his colleagues.

Throughout this book the author's originality and "fresh ideas" are readily discernible. Even if some readers might not agree with some generalizations presented here, the fact still remains that in our field of research there are no laboratory tests to provide any sort of measurement for intellectual judgments. This situation is more accentuated in matters dealing with classical Islam simply because there are no primary sources at hand. Aware of this problem, Professor Dunlop closes his book with this statement: "An overriding consideration, however, which makes many generalizations about the Arab empire mere personal opinions, is the absence of archives which, except for such documents as happen to be included in

the works of historians or others, are almost totally lacking" (p. 267).

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DEREK HOPWOOD, editor. *The Arabian Peninsula: Society and Politics*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 320. \$11.50.

In 1968 Britain announced that after a century and a half she was about to lay down her political responsibilities in the Persian Gulf. Middle Eastern specialists reacted to this news in characteristic fashion, and scholarly conferences featuring papers by an array of genuine (and a few not-so-genuine) specialists on Arabia and the Gulf became something of a fad. This volume is the record of one of these conferences—in this case one held early in 1969 under the joint sponsorship of the School of Oriental and African Studies of the University of London and the Middle East Centre of St. Antony's College, Oxford.

In the technical sense, the book benefits from skillful editing by Derek Hopwood, Oxford's Middle Eastern bibliographer. The work consists of fourteen articles organized around five themes: bibliography, history, political development and international relations, sociology and culture, and economics; it also contains three informative—if slightly dated—statistical tables and a useful index. Although there are some unfortunate efforts included among the individual papers, the majority are, at the very least, workmanlike summaries, and a few stand out as original, important contributions. J. C. Wilkinson, an Oxford historical geographer, has written an imaginative yet solid discussion, "The Origins of the Omani State," based upon extensive knowledge of little-known Arabic materials. R. D. Bathurst's well-documented study produces a closely knit explanation of Oman's emergence as an Indian Ocean maritime power and the effects of this development upon the country's internal political history up to the early eighteenth century. The Oxford sociologist, P. A. Lienhardt, in "Some Social Aspects of the Trucial States," has written an interesting piece, which focuses upon the status and role of women in contemporary eastern Arabian society. Also, Frank Stoakes, a Manchester political scientist, has

provided a thought-provoking analysis of current social and political change in eastern Arabia.

Unfortunately, the lively bibliographical article by Hopwood that opens the volume is too narrow in scope to provide an essay broad enough in concept to undergird this book, which aspires to furnish scholars with a sophisticated introduction to the Arabian scene. The book's foreword expresses the hope that it "will not only provide students of the Middle East with an introduction to the social and economic and political problems of Arabia and the Persian Gulf, but also generate wider interest in further research and study of the area." In my judgment, the first-mentioned hope is not realized because the work is a qualitatively uneven mosaic displaying large gaps in coverage. Moreover, much of the information, analysis, and speculation it presents became dated during the long interval between the original appearance of the papers and their ultimate publication. The second, more modest and realistic purpose may well be achieved because of the excellence of some of the individual contributions.

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M. A. COOK. *Population Pressure in Rural Anatolia, 1450-1600*. (London Oriental Series, volume 27.) New York: Oxford University Press. 1972. Pp. 118, 3 maps. \$20.00.

It has become traditional for Ottomanists to deplore both the state of the art, which is singularly underdeveloped, and the state of the archives, which impedes development. In this brief but brilliant essay, M. A. Cook offers an example of the kind of work that will ultimately end this circular dilemma and make it possible to write the social and economic history of the Ottoman Empire with less diffidence and more precision.

The stated purpose of this study is to test the thesis advanced by F. Braudel (*La Méditerranée et le Monde méditerranéen à l'époque de Philippe II* [Paris, 1949; rev. ed. 1966]) that emergent overpopulation resulted in a decrease in grain exports from the eastern Mediterranean in the late sixteenth century. In fact, the question of Mediterranean trade is not really

considered. As the author indicates, there are no reliable measurements of Ottoman grain production and exports. Moreover we note that increased land values, land expropriation, and the use of marginal lands could indicate growing profitability of agricultural production rather than overpopulation.

It is the second aspect of Braudel's thesis, the demographic problem, that is of primary concern. Through a meticulous examination of *Mühimme* surveys for three districts in Anatolia, the author attempts to utilize fiscal materials to establish indices of population growth and population pressure. He then presents a provocative essay on the possibilities of a correlation between rural disorders in Anatolia, specifically the Celali uprisings, and emergent overpopulation. While we question the elimination of certain categories from the survey, especially *vakf* and *mülk* lands, the limitations of the materials and the methodology are clearly indicated: the boundaries of the problem are rigorously defined, and every effort is made to optimize the validity of the data. The results of the study are statistically inconclusive. The limited materials available, the ambiguity of the data, and the inadequacy of the resource base make the evaluation and analysis of social and economic developments problematic. This is not to criticize but to agree with Mr. Cook. We cannot proceed to articulate change in the Ottoman Empire without further research of the quality reflected in this volume.

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WILLIAM L. CLEVELAND, *The Making of an Arab Nationalist: Ottomanism and Arabism in the Life and Thought of Sati' al-Husri*. (Princeton Studies on the Near East.) Princeton: Princeton University Press, 1971. Pp. xvi, 211. \$8.50.

Mustafa Sati' al-Husri (1880-1968) is certainly a worthy subject for an intellectual biography. An Arab of an Aleppo family, his early career was spent as a loyal member of the Ottoman elite: teacher, provincial administrator, educational reformer, and publicist promoting the multinational Ottoman bond. Then, when World War I shattered the Empire, he left Istanbul and gave his loyalty to the Arabs, serv-

ing as educational planner but, most important, becoming one of the earliest and most influential spokesmen of Arab nationalism. Al-Husri's life and ideas, therefore, are deserving of study on several levels: as a spokesman of Ottomanism, as an ideologue of Arabism, and as a case study of the readjustment of identity that many Arabs of his generation had to undergo with the dissolution of the Ottoman Empire.

Professor Cleveland's work gives most of its attention to the second phase of al-Husri's activities, that of advocate of Arabism. Here the book is excellent. Al-Husri's predominantly linguistic concept of nationalism, his use of historical arguments to prove both his theory and its applicability to the Arabs, his attacks on narrower concepts of nationalism, the minimal place given to Islam in his writings, and finally the instrumental aspects of his teachings with their emphasis on selective education in the service of nationalism and on the necessity of inculcating the younger generation with the virtues of solidarity and sacrifice—all this is summarized lucidly from a comprehensive examination of al-Husri's writings and then analyzed critically and judiciously. While the intellectual weaknesses and the practical, "manipulative" goals of al-Husri's doctrines are noted, there is also a scrupulousness of analysis that avoids squeezing the man into intellectual categories where he does not fit. Al-Husri was a complex man, if not a terribly sophisticated thinker; Professor Cleveland's treatment does justice to that complexity.

Al-Husri's Ottoman phase and his shift from Ottomanism to Arabism receive less attention. While the few pages summarizing his pre-World War I thought are valuable, one suspects that much more could be said about his early career. The ideas offered on al-Husri's reorientation in loyalty after the war are fascinating, however. For this shift Professor Cleveland seems to suggest largely an explanation of expediency: that al-Husri, who knew, but rejected, linguistic theories of nationalism in his Ottoman phase (for example, he criticized Ziya Gökalp's ideas on Turkism in 1911), came to see the linguistic bond as the only possible basis of Arab identity after the shattering of the Ottoman Empire. This analysis (fairly, if not totally, convincing) raises important ques-

tions about the depth and meaningfulness of modern linguistic-historical Arab nationalism. Was (is) belief in Arabism subject to change, as al-Husri's belief in Ottomanism itself gave way to Arabism after World War I? Was (is) the espousal of an eternal Arab nation but a rationalization for the more practical position that Arab unity is the only way to Arab survival in the modern world, as seems to have been the case with al-Husri? This work does not address these questions directly, but its probing of the thought of Sati' al-Husri provides fertile material for the entire subject of the commitment of nationalists to their nation.

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JACOB M. LANDAU. *The Hejaz Railway and the Muslim Pilgrimage: A Case of Ottoman Political Propaganda*. Detroit: Wayne State University Press. 1971. Pp. 294. \$15.95.

Jacob Landau of Hebrew University is to be commended for providing us with a transcription and an English translation of *al-Sa'āda al-nāmiya al-abadiyya fī'l-sikka al-ḥadīdiyya al-Hijāziyya* (*The Increasing and Eternal Happiness—the Hejaz Railway*) by Muḥammad 'Ārif Ibn al-Sayyid al-Munīr al-Ḥusaynī'l-Dimashqī (Library of Istanbul University, Arabic MS 4780). An introduction, a map, a glossary, facsimiles of the frontispiece and two pages of the manuscript, five pages of photographs, and an index increase the volume's usefulness.

As the title he chose for this volume indicates, Professor Landau is principally interested in 'Ārif's manuscript as an example of Muslim-oriented Hamidian political propaganda aimed at the Arabs. His introduction deals with the late nineteenth- and early twentieth-century phenomena of Ottoman railroad construction, particularly the Hejaz Railway, and of Ottoman propaganda among the Arabs. It also contains information concerning the manuscript and its author in which Professor Landau calls attention to the valuable description of the pilgrimage provided by 'Ārif who "wrote with the knowledge of an insider" (p. 27).

An examination of the text suggests that it

was this detailed account of the pilgrimage that 'Ārif wrote first. In it he describes the caravan route and its stations, the Bedouins along that route, the ceremonies in Istanbul and Damascus attending the departure and return of the pilgrimage caravan, the caravan's mail service, the religious rituals in Mecca and Medina, and the rationale for the pilgrimage.

In 1900, while in Istanbul, 'Ārif prepared and presumably presented to Sultan Abdül Hamid II a work intended to refute the arguments raised against the construction of the Hejaz Railway. After a preface in which he frankly announces this intention, 'Ārif has inserted his account of the pilgrimage (sections 1–10) with an occasional reference to the proposed railroad seemingly added, usually at the beginning or the end of a section. This is followed by several sections (11–17) devoted entirely to extolling the potential benefits and advantages of the Hejaz Railway to Muslims in general and to the Syrian and Hejazi Bedouins in particular. Among these are "populating the country, restoring life to the servants [of Allah], serving the Two Shrines [Mecca and Medina], assisting those desirous to visit both of them, promoting . . . commerce, bolstering . . . agriculture, and maintaining the political balance of the . . . Arab lands" (p. 42).

This volume thus supplements our factual knowledge of the Syrian-Hejazi pilgrimage at the turn of this century, as well as giving us a pro-Ottoman Arab's views on the Hejaz Railway.

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HASSANEIN RABIE. *The Financial System of Egypt, A. H. 564–741/A.D. 1169–1341*. (London Oriental Series, volume 25.) New York: Oxford University Press. 1972. Pp. xii, 242. \$21.00.

This extensively documented study is a detailed reconstruction of the sources of revenue (chapter 3), financial administration (chapter 4), and the *iqṭā'* (an administrative grant of revenue, usually based upon land) system (chapter 2), as well as a survey of the monetary changes (chapter 5) during the Ayyubid dynasty and the first century of Mamluk rule in Egypt. The

breadth of the medieval Arabic material and the Geniza documents that Professor Rabie has culled for their isolated and often obscure data is reflected in his extensive bibliography, the numerous footnotes, which are conveniently placed with the text, and the introductory chapter, which surveys the value of the various types of sources. Only an examination of the traditional chronicles is missing.

The caution with which Rabie uses his source material is immediately apparent in the body of the work. In each chapter the author brings together all the available information within appropriate subsections, for example, the size and value of the *iqṭāʿ*, taxes on vices, confiscations, tax farming, and so forth. Not only is there new data, but their cumulative effect is to give us the most detailed account of any financial system for medieval Islamic history.

There are two main weaknesses in the work. The author has been tempted to allow his data to speak for itself. On topics such as the importance of the cadastral survey of 1315, the changing role of the wazirate, or the reasons for the decrease in the supply of Egyptian gold currency under Saladin, Rabie's statements have applications far beyond the specific topic being discussed. But, too often, the reader must draw his own conclusions, whether it be about the impact of a particular tax or monetary change or about the general structure and dynamics of Ayyubid-Mamluk financial systems as a whole.

Rabie's caution in avoiding possibly imprecise Western terminology such as fief and feudalism is commendable, but I believe he went too far. For example, Rabie writes that "the *ajṇād al-halqa* resorted to *nuzūl* to have their *iqṭāʿ*'s changed to pay" (p. 56). The first Arabic term is defined on page 40, note 2, which, unfortunately, is not listed in the index under *ajṇād al-halqa*; the second term can only be translated by context; and the third should be left as *iqṭāʿ*. This illustrates the second and more serious problem, the lack of a glossary or translation of all the Arabic terms employed in the index and text. Ultimately it will have to be the specialist or a most patient and careful reader who will be able to extract the extensive and fundamentally important information that

Dr. Rabie has so carefully gathered and systematically organized.

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JANET L. ABU-LUGHOD. *Cairo: 1001 Years of The City Victorious*. (Princeton Studies on the Near East.) Princeton: Princeton University Press, 1971. Pp. xiv, 284. \$25.00.

It is generally recognized today that Cairo is one of the world's great cities, the largest by far on the African continent or in the Middle East. What is seldom appreciated is that in the fourteenth and fifteenth centuries, before shifting trade routes ended its role of entrepôt in East-West trade, its population was at least double that of any city in Europe. Mrs. Abu-Lughod's study details the later vicissitudes of the medieval city, through its long decline under Ottoman and Mamluk rule, and its more recent status as a kind of *casbah* or *medina* contrasting with the new Europeanized quarters constructed along its western edge on the former floodplain of the Nile. Since the author's main concern is sociological, she does not attempt to rehearse Cairo's long record of distinguished contributions to Islamic theology, law, and science; the thousand-year-old mosque-university of al-Azhar is scarcely mentioned. Within the restrictions she has set for herself, however, Mrs. Abu-Lughod does a skillful job of translating the raw data of her sources, particularly the census returns of 1947 and 1960, into informative and readable prose. The prospective reader should not be put off by the battery of tables and charts nor by the note on methodology, tactfully postponed to an appendix. There are helpful maps and more than a hundred instructive illustrations, many of them from photographs by the author. The results are a handsomely produced volume and a series of well-delineated portraits of the immensely varied districts constituting this fascinating city. Everywhere Mrs. Abu-Lughod seeks to convey the nature of the inhabitants, from the well to do in the high-rise apartments of Garden City and Zamalek along the Nile to their neighbors, the slum dwellers of overcrowded Bulaq (where the population density exceeds 100,000 per square kilometer!).



and from the middle-class and bureaucratic families of the planned communities of Helio-polis and Nasr City to the tens of thousands of Egyptians, many of them evacuees from the Suez Canal zone, who have made makeshift homes among the tombs and shrines of the grotesque Cities of the Dead.

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JACQUES BERQUE. *Egypt: Imperialism & Revolution*. Translated by JEAN STEWART. New York: Praeger Publishers. 1972. Pp. 736. \$38.50.

"You strive towards a sublime goal, but you never reach it. And who then will reach it? An agonizing question, faced with which Fate veils her face. . . ." (*Faust*, part 2, quoted in the author's preface, p. 26). To these lines, Jacques Berque might well have added the Angel Chorus at the end of *Faust*, part 2: "Gerettet ist das edle Glied/Der Geisterwelt vom Bösen,/Wer immer strebend sich bemüht/Den können wir erlösen." (Saved is this noble soul from ill./Our spirit-peer. Who ever/Strives forward with unswerving will,—/Him can we aye deliver. . . .)

These latter lines from Goethe sum up the "message" of this magnificent book, full of hope despite the sordidness of the past and prophetic of what could happen if the lessons of the past continue to be ignored. "Can it be," writes Berque (pp. 29–30), "that the new being now struggling to emerge from the ordeal amidst violence and misunderstanding is really recreating the synthesis of which Western man had lost all hope? In any case, that is what he must needs do. The alternative is his downfall." The French edition of this book was published in 1967, that most fateful year for Egypt and the entire Arab world. The revolutionary generation who created Nasser's Egypt did not profit from these lessons; nevertheless the "message" is still a valid one and the hope still remains.

The central question posed by Berque is: "Under what circumstances, according to what correlations, does liberation result dialectically from dependence?" (p. 25). He has focused on Egypt during the period between the British occupation in 1882 and Black Saturday of 1952, on the eve of the July revolution. He

effectively demonstrates that in this dialectical process, colonization and decolonization "are not so much successive phases in time as rival phases. . . ." Failure to see colonization and decolonization as rival phases has led other historians to deal consecutively with events that were in fact occurring simultaneously, tending thereby to disguise the inner reality of Egyptian history and to attribute meaning to events that have no meaning without reference to this inner reality. Berque does not make the same mistake. For this reason, his book provides the most comprehensive picture of this period of Egyptian history yet to be published. Furthermore, it points to the direction that must be taken in all future works on modern Egypt and other areas of colonization and decolonization. It is not an easy assignment, certainly not for those who lack that special perception Berque has shown in all of his books dealing with the modern Arab world—a perception that can only come from long acquaintance with and an appreciation of the Arabs.

Lacking the range of monographic materials available to social historians of Western societies, Berque relies heavily on his training as an Orientalist and sociologist to extract from many different sources (literature, newspapers and journals, archival materials, personal observation, and innumerable conversations with Egyptians) the evidence required for the creation of a social history of Egypt during a period of total disruption. In this period everything deemed permanent in Egyptian society fell under attack; no adequate substitutes replaced what was destroyed. Western conceptions introduced in the process of colonization were indiscriminately accepted by the Egyptian ruling elite and by Egyptian intellectuals, an acceptance often skin-deep and consequently uncreative.

With a broad brush and in lavish colors Professor Berque paints a picture enormously exciting in its variation. He begins with a description of Egyptian society just before the age of colonization, a society already undergoing a process of change but nonetheless retaining its traditional contours. The period of colonization interrupted the organic evolution of Egyptian society by the introduction of new factors. Whatever good British rule brought to Egypt must be balanced against the disruptions



it caused. Deluded by their accomplishments, the British never properly assessed their shortcomings. In the end, they created conditions that produced a mood of violence among the Egyptian peasantry, that led to the growth of a nationalist movement among the intellectuals, and that made the revolution of 1919 by all strata of Egyptian society an inevitability.

The period between the wars turned out to be a period of lost opportunities, not so much because of the three-cornered political struggle conducted by the palace, the British, and the Wafd, but because of the failure of all to come to grips with the essential problems already crowding in. One could recognize already the economic decadence, the population explosion, the inequities in the distribution of wealth, the growing alienation of the young, the emergence of political extremism, and the decline in the influence of the religious authorities, especially of the more moderate elements. For a brief moment, a reversal of these trends appeared possible with the ascent to the throne of the young king, with the Anglo-Egyptian Treaty of 1936, with the end to the capitulatory regime made possible in 1937 by the Montreux conference, and by the entry of Egypt into the League of Nations.

World War II shattered all illusions. It was not solely the continuation of the British occupation that made everything go wrong; by this time, the process of decolonization was nearly irreversible. Rather, it was the inadequacies, opportunism, and greed of the ruling clique. Their failures were monumental and the consequences of these failures predictable. Professor Berque's judgments are their harshest at this point. He spares no one.

The immediate postwar era produced no improvement. Rather, things went from bad to worse. Egyptian reversals over the Sudan and in Palestine, a cholera epidemic, the degeneration of the king and of the politicians, the exigencies of cold war politics—all combined to produce the explosions of early 1952 with which the author concludes his book. The process of colonization and decolonization had moved into a new phase. An opportunity for a new beginning would soon be available to those elements produced by a vastly changed Egyptian society. Professor Berque does not carry his analysis into the revolutionary era,

nor does he state directly in the body of his book his judgments as to its accomplishments. It is clear, however, that he does not consider the struggle against the colonizers the central problem of the future. More important would be the redirection of the decolonizing efforts along lines more deeply rooted in the Egyptian soil. "Is it sufficient to adopt, after having endured, the hypotheses of Western civilization as the sole form of progress? Rather, should not every civilization go forward in its own way to discover the technological stages of the future?" (p. 26). "He [the Egyptian] must reconcile his specific personality and his universal humanity, both within himself and in the eyes of the Other. While deeply rooted in the soil, he must aspire towards a supremely rational future. He must ensure that this crisis of his personality and that of society as a whole correspond to one another in purpose and in significance, and join in commitment to a single end. He knows that he must belong to the world, or cease to be" (p. 30).

Although this book is brilliantly conceived and brilliantly executed, it suffers from a few defects that might easily have been avoided. The form of indexing the work proves an obstacle to easy reference rather than an aid, and the absence of a full bibliography at the end of the book is deplorable. Numerous references are made to sources within the body of the work without any indication of where they may be examined. It is possible that Professor Berque was not always in a position to disclose their location, but even that fact would have proved useful to know. But to carp on the lack of adequate scholarly paraphernalia or on the errors that inevitably occur in so precocious an attempt of synthesis seems out of place in a review of so eminent a book by so distinguished an author.

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JACQUES COULAND. *Le mouvement syndical au Liban (1919-1946): Son évolution pendant le mandat français de l'occupation à l'évacuation et au Code du travail*. Preface by JACQUES BERQUE. Paris: Éditions Sociales, 1970. Pp. 453.

This is an attempt to chart the evolution of labor unionism in Lebanon during the French

Mandate, from 1919 to 1946, the year of the French evacuation and the passage in Parliament of the *Code du Travail*. The book discusses the manner in which the various communities in this nation of confessional and ethnic mosaic have participated in or reacted to the dynamics of the workers' struggle and demands; to what extent and how Lebanese labor unionism was inspired by external models, be they European or Egyptian; how much progress toward the transformation of Lebanese and Near Eastern societies resulted from the sophisticated debate that labor unionism sparked; and in what way such debate was more conducive to change than the usual type of intercommunity bickerings.

Couland's work is a serious research endeavor, not only into public and private archives and the news media but also utilizing the wealth of oral testimony of historical witnesses. *Le Mouvement Syndical au Liban* suggests a constant interplay between two types of institutions—the unions, a kind of association established by workers determined to protect their professional interests, and the Mandate, a type of colonial domination with international blessings. The former is concerned with antagonistic social groupings within a global society, the other between two global societies—one dominated, the other dominating. These two institutions, existing on different levels and with different aims, interfered on a common ground, that of a definite socioeconomic formation—industrial capitalism.

Jacques Couland has discussed the evolution of unionism in Lebanon historically, using the following stages: (1) When the French occupied Lebanon in 1919, unions were nonexistent, and the working class was still embryonic and scattered. Lebanon was not yet a political entity. Autonomy, on a confessional basis, for Mount Lebanon nestled as it is in an Arab-Islamic region was antieconomic. Yet in 1921 the *Parti du Travail* was constituted. (2) 1926 ushered in a period where transitional domination was replaced by a permanent one. Economic difficulties and the Syrian revolution created a favorable climate for spontaneous labor strikes; the "drivers" were reorganized, and their strikes were impressive because of the important role these chauffeurs played in the regional economy. Notables and nationalist leaders sympa-

thized in an attempt to enlist their leaders and thus enlarge their clientele. Similarly, the typographers' association transformed itself into a union directed by workers but seeking alliances with notables and political leaders. (3) The world economic crisis worsened the situation by 1936 and the pace of industrialization slowed down. On the eve of World War II a federation of labor was at hand—the *Comité d'Union Syndicale* with a quasi-legal status. (4) Progress in unionism was interrupted during the war. The first phase (1939–41) of the war manifested itself as a police repression against both the nationalist and labor movements. In its second phase (at least until 1943) it changed direction. Free France abolished the Mandate—courting labor in Lebanon, just as in France, as a means of combating Germany. The unions were reconstituted around the *Comité d'Union des Syndicats*.

The history of labor unions in Lebanon thus appears from this impressive study to follow the vicissitudes of the political and economic history of that nation, itself conditioned by the dialectic of the relationship between colonizer and colonized. *Le Mouvement Syndical au Liban, 1919–1946* is indispensable to all students of the Near East and of Lebanon. I recommend it explicitly and hope that it will soon be translated into English.

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#### AFRICA

ATTILIO GAUDIO. *Allal El Fassi: Ou l'histoire de l'Istiqlal*. Preface by JACQUES BERQUE. Paris: Éditions Alain Moreau. 1972. Pp. 365. 35 fr.

The title is misleading, for this book is not a history of Morocco's Istiqlal party; it is nonetheless a valuable contribution to the presently insufficient body of knowledge available to the West concerning a historically and strategically significant part of the African continent. In it Attilio Gaudio, an Italian journalist who has been a friend and confidant of Allal El Fassi for twenty-five years, presents a comprehensive compendium of the contributions of El Fassi to Istiqlal party doctrine and policy. His account is presented substantially in El Fassi's own words, for Gaudio relies upon numerous

(and frequently extended) quotes from El Fassi's writings and speeches. The book, written in French, contains 240 pages of adequately documented text, an additional 125 pages of annexes, and 17 documents, many of them translated from Arabic. Unfortunately, there is no index.

Allal El Fassi, a pioneer in the Moroccan nationalist movement, has exerted an important influence on the policy of the Istiqlal party since its organization in 1944, and as president of the party since 1960, perhaps a dominant influence. His has been an idealistic vision of the participation of Moroccans in a democratic society guided by the principles of Islam. According to El Fassi, nationalism, as the rejection of European colonialism, is a stage through which Morocco must pass before joining a much larger Islamic brotherhood. El Fassi advocates the Arabization of Moroccan culture, specifically education; the rejection of Western capital investment and influence; the nationalization of the mining, banking, insurance, and other industries; and the redistribution of land to the actual cultivators—in short, the Arab socialism of the Middle East proper.

The weakness of the book lies in Gaudio's failure to account for El Fassi's interaction with other leaders of the Istiqlal party, like Ahmed Balafrej. El Fassi's attitude toward government was theoretical, not practical. Almost certainly his participation would have been welcomed in any of the cabinets formed in the period between the achievement of independence in 1956 and the proclamation of a state of exception by King Hassan II in 1964; except for an eighteen month period between June 1961 and January 1963 he preferred to remain "in opposition"—the sort of opposition to which the king referred following the army mutiny at Skhirat in July 1972 as a "spirit of criticism" rather than the "critical spirit" needed in the nation.

El Fassi's fixation on his concept of the ideal not only obviated the personal contribution he might have made toward the development of government in an independent Morocco, but fragmented and weakened the Istiqlal party as well. Traditionalists, Berbers for the most part, alienated by the apparent Istiqlal willingness in 1954 to accept a monarch other than the exiled Mohammed V, fashioned in the Liberation

Army an effective force for independence. This force was later politicized in the *Mouvement Populaire*, led by Majoub Aherdane and Mohammed El Khatib. Modernists, led by Mehdi Ben Barka, Abderrahman Bouabid, and Abdallah Ibrahim, split off into the *Union Nationale de Forces Populaires*. Ahmed Balafrej threw his lot with the monarch, although he retained his party membership. Others, frustrated by El Fassi's visionary and unproductive leadership, dropped out of political life. But, whether positive or negative in influence, Allal El Fassi remains an important figure in North African politics; he deserves to be better understood, and Gaudio's book contributes to that understanding.

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HENRI BRUNSCHWIG, with the assistance of GEORGETTE LAGARDE and JAN VANSINA. *Brazza explorateur: Les traités Makoko, 1880-1882*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Documents pour servir à l'histoire de l'Afrique équatoriale française. Second series, Brazza et la fondation du Congo français, number 2.) Paris: Mouton & Co. 1972. Pp. 298. 59 fr.

This is the second volume in a three-volume series of documents entitled "Brazza et la Fondation du Congo Français." The first volume, which appeared in 1966 under Brunschwig's direction, covered Brazza's first mission to Equatorial Africa between 1874 and 1879. The third volume, which was published in 1969 under the editorship of Cathérine Coquéry-Vidrovitch, dealt with Brazza's third mission from 1883 to 1885. The present volume contains materials on his second mission, 1880 to 1882. It is divided into three sections: the travel diaries of Brazza, the correspondence between him and the French Committee of the International African Association, and official documents. Two of the three notebooks in section 1 have already been published in the *Cahiers d'études africaines* (no. 1 [1965], 5-56; no. 2 [1966], 157-227). They cover, respectively, the negotiation of the treaty with the Makoko (January 22 to March 15, 1882) and the trip from Franceville on the Ogowe River into the Congo Basin (June 22 to August 16, 1880). These notebooks reveal that Brazza

consistently sought to extend French political control into the Congo Basin and that he negotiated his treaty with the Makoko during palavers lasting nearly two weeks. The two notebooks are reproduced here together with the notebook on Brazza's return via the Niari-Kwilu (August 28 to October 3, 1880). Jan Vansina's excellent notes on African peoples and geography contribute much to an understanding of the diaries, as do Georgette Lagarde's biographies of the French participants.

The past fifteen years have seen a re-examination of the origins and course of the late nineteenth-century scramble to partition tropical Africa. In his earlier studies Brunschwig has contributed a good deal to a review of the French role. Through the editing of these documents he makes it possible for larger numbers of students to examine some of the important French sources for these developments. In addition he allows us to study the role of Brazza himself, who did so much to extend the French empire into Equatorial Africa and to introduce a rule that was to prove so disastrous for millions of blacks even within his own rather brief lifetime.

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JOHN S. GALBRAITH. *Mackinnon and East Africa, 1878-1895: A Study in the 'New Imperialism.'* (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1972. Pp. viii, 253. \$17.50.

In this scholarly and eminently readable work, John Galbraith attempts to shed new light on the process and politics of the late nineteenth-century British expansion in East Africa through an examination of the ill-fated career of the Imperial British East Africa Company and the activities of its founder, William Mackinnon. Galbraith is ultimately successful, and historians of Africa and the British Empire alike will benefit from the new material he has collected. He shows in painful detail the imperialist foot-dragging of the Foreign Office in the 1880s, wanting to preserve British interests in East Africa, but anxious to maintain a cordial relationship with Germany, and above all reluctant to commit any of the taxpayers' money. Galbraith shows convincingly that it was the fear of a German takeover in Bu-

ganda—thus threatening the British presence in Egypt and the Sudan—that finally sparked the decision to act.

Under these circumstances, the chartering of a private company seemed an ideal solution. Mackinnon and his friends could simultaneously develop the commercial potential of East Africa and carry out the strategic and political goals of the Foreign Office by establishing the British presence in Buganda and the upper Nile. In the end, of course, they could not. The company failed in both its political and commercial objectives, and its failure was probably inevitable. East Africa lacked the kind of resources that could have yielded significant profits in the short run, and the "precipitate rush into Buganda" fatally overextended the company's means. Galbraith argues, however, that in the long run strategic considerations outweighed commercial gain in Mackinnon's thinking as much as in Salisbury's. While the company directors were originally attracted by the promise of commercial profit, their motivations ultimately derived from a sense of patriotic imperialism: "The directors believed that . . . the greatest dividends would not be in personal profit but in securing to Britain territories which could be valuable economically and strategically" (pp. 238-39).

Galbraith's book, then, is a welcome addition to our knowledge about British imperialism in Africa, and it takes its rightful place alongside earlier studies of Rhodes and Goldie, of company rule in western and southern Africa, and of the politics of European expansion. Some will regret that Galbraith did not say more about the African part of the story. We learn nothing about the operations of the company on the ground, for example, or its interaction with Kikuyu and Ganda. African rulers such as Mwanga and Barghash lack the reality of historical actors in this account: they exist only in correspondence preserved in P.R.O. files. It is perhaps unfair to fault a scholar for not adventuring beyond the accepted confines of his discipline, yet in this sense the complete history of the Imperial British East Africa Company remains to be written. This narrowness of scope is the major disappointment in an otherwise promising book.

MARGARET HAY  
Wellesley College

## ASIA AND THE EAST

CARMEN BLACKER *et al.* *Tradition and Modernization in Japanese Culture*. Edited by DONALD H. SHIVELY. Princeton: Princeton University Press, for the Conference on Modern Japan of the Association for Asian Studies. 1971. Pp. xvii, 689. \$14.50.

The last decade and a half has been an enormously fruitful period for Anglo-American scholarship on modern Japan. Much of this contribution to our increased understanding of Japanese society, culture, and history can be attributed to the maturation of a postwar generation of scholars supplied with adequate research funds (funds that are unfortunately now drying up); but another factor of great significance has been the widespread acceptance, if often only implicitly, of a more sophisticated conceptual framework for treating modernization. This paradigm, refined in the literature of the postwar social sciences, has provided a set of shared perspectives and concepts of considerable usefulness. The series "Studies in the Modernization of Japan" has been important both in presenting some of the best of recent research and in pointing the direction for further efforts.

The present volume, the fifth and penultimate in the series, is in some ways the most intriguing, for it is an explicit attempt to extend this view of modernization to culture as understood by those who would defend the autonomy of the arts and the humanities. The attempt, however, is only partially successful. There are hints of the frictions generated by such an endeavor in Professor Shively's preface: "It would divert us from our primarily humanistic interests to reopen problems of definition and theory in the studies of modernization. But to attempt to relate cultural changes to modernization taking part in other sectors of a society is to venture into an area that has been investigated very little" (p. xv). John Rosenfield, in an interesting tour of Western-style painting in the first decades after the Meiji Restoration of 1867, feels constrained to warn us that art is more than merely a "social or economic phenomenon" (although he does offer some insights into how they are interrelated). Valdo H. Vigliemo, in his admirably written essay on Nishida Kitaro (twentieth-century Japan's fore-

most philosopher), raises the issue more directly by admonishing those who suffer from "uncritical faith in the social sciences, particularly social psychology," and insisting that we come "to grips with the true thrust of Nishida's thought, *qua* thought" (p. 508n.). William Malm's "The Modern Music of Meiji Japan," despite some tantalizing glimpses of music as a part of popular culture, basically refuses to confront the issue at all by concluding that "if modernization means 'conforming to present [*sic*] usage, style, or taste,' then only parts of the Meiji musical scene can be called truly modern" (p. 300). The point, of course, is not that Vigliemo's admonition about uncritical faith is gratuitous, but rather that the facile generalizations and fallacies of reductionism to which he and others (including myself) would object can be corrected only by either confronting them directly or pointing the way to an alternative paradigm.

On the other hand, there is much of interest to be learned from these discussions of painting, music, and philosophical thought; and, moreover, the volume does include a number of excellent articles that make better use of the conceptual tools implied in the title. Our level of sophistication is raised substantially by those addressed directly to the triadic interplay between traditional culture, Western influence, and modernization (as distinct from Westernization): for example, Eugene Soviak's careful, concise analysis of the impressions of Western civilization brought back by the 1871 Iwakura Mission; Michio Nagai's and Donald Shively's articles on "Japanization" as a reaction to government policies perceived as politically authoritarian as well as culturally threatening. Our understanding of the social pathology and crises of identity associated with modernity is further enhanced by the contributions of Donald Keene on the cultural effects of the Sino-Japanese War of the 1890s and Carmen Blacker on millenarian religious movements, and by the treatment of the lives and works of leading (if, as the editor tells us, not necessarily "representative") literary figures by Professors Brower, Hibbett, McClelland, Ortolani, and Seidensticker. Again, however, there are signs in the latter group of ambivalence in their approaches to the subject matter; one of the few solid points regarding modern fiction upon



which there is a common consensus among these literary scholars is the change in diction and grammar to incorporate popular speech. The final article by Roy Andrew Miller attacks the problem of linguistic modernization with precision as well as wit. His conclusion suggests that the Weberian concept of rationality and modernity is far too limited to encompass the complexities of change in the Japanese use of respect or honorific language.

In sum, the quality of the individual monologues is high, and the editor labors energetically in his preface and introductions to bring the group discussion back to the common themes. If at times one feels frustration at the absence of more sharply focused dialogue, it may be because too much was conceded in the ground rules at the outset.

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S. L. VAN DER WAL, editor. *Officiële bescheiden betreffende de Nederlands-Indonesische betrekkingen, 1945-1950* [Official Documents Concerning Dutch-Indonesian Relations, 1945-1950]. Volume 1, 10 aug.-8 nov. 1945; volume 2, 9 nov.-31 dec. 1945. (Rijks Geschiedkundige Publicatiën, Minor Series, numbers 36 and 37.) The Hague: Martinus Nijhoff. 1971; 1972. Pp. xxiv, 616; xix, 628.

The severely plain title of this series, "Official Documents Concerning Dutch-Indonesian Relations, 1945-1950," should not mislead the reader. Its subject is not some episode of foreign relations but the tragedy of the Dutch effort, amid the storm of the Indonesian revolution, to restore their lost Netherlands Indies. Nor are its documents confined to foreign relations, though the many-sided diplomacy of the struggle is thoroughly covered here. Professor van der Wal also ranges widely through private letters in Holland, newspaper stories, and especially the reports of Indies officials to survey the larger setting in which the political perceptions and policies of the leading Dutch authorities were formed and changed.

It is an ambitious enterprise. The two present volumes—each with six hundred pages in fairly small type, along with detailed notes and two indexes—cover only four and a half months of the five years. Even allowing for some quieter periods later it will take a good

dozen more such volumes to complete the story. The task is in the best of hands; in Professor van der Wal are combined not only the command of subject familiar from his earlier collections on prewar Indies politics and education, but also a formidable capacity for work, the establishment standing so important in a politically sensitive enterprise, and the historical imagination necessary for a grand design.

The 576 documents in these two volumes, plus a hundred more included in the footnotes, are selected from numerous Dutch and Indies departmental archives. The largest category is communications between Lieutenant Governor-General van Mook and Colonial Minister Logemann. But much space is also devoted to cabinet meetings in The Hague; correspondence with the Dutch ambassadors in London, Washington, and Australia; conferences and correspondence connected with Lord Mountbatten's Southeast Asia Command that was charged with reoccupying the Indies after the Japanese surrender; and Indies officials' field reports, recommendations, and memos on contacts with Indonesians. About five per cent of the documents are in English, the rest in Dutch. Language alone is likely to deter all but specialists, which is unfortunate because there is much here of importance for students of post-war British, United States, and Australian policy and of decolonization generally.

For students of recent Dutch and Indonesian history the series will be, quite simply, indispensable. The present volumes have a special appeal for, covering a period whose strong natural story-line has been enhanced by deft editing, they are absorbing and at times exciting reading. By the end of these decisive months, after much noisy clashing of gears, the Dutch apparatus is in working order and has settled on a basic policy of trying to push moderate Indonesians into autonomy within the Dutch Kingdom. The reasoned doubts of many high Indies officials (volume 2), including van Mook, have been set aside, as much by van Mook himself as by The Hague; the poignant appeal of an Indonesian Indies official (Raden Singer, volume 1, pp. 521-23) has been ignored; the tragedy can now unfold over the years.

JOHN R. W. SMAIL  
University of Wisconsin,  
Madison



## UNITED STATES

SAM BASS WARNER, JR. *The Urban Wilderness: A History of the American City*. New York: Harper and Row, 1972. Pp. xvii, 303. \$12.50.

ARTHUR MANN *et al.* *History and the Role of the City in American Life*. (Indiana Historical Society Lectures, 1971-1972.) Indianapolis: the Society, 1972. Pp. 65. \$1.50.

These two volumes, similar only in their focus on the urban experience, testify to the vigor and variety of American urban history. Warner's book skillfully examines the entire range of city development and its human and social consequences in the United States. The second volume, a series of three lectures sponsored by the Indiana Historical Society in 1972, probes selected aspects of urbanism and urban history.

Despite (or perhaps because of) a somewhat unorthodox organization and a personalized treatment of subject matter, Warner's *The Urban Wilderness* is a creative and sophisticated urban history. It draws upon an impressive array of writings in history, demography, political science, economics, law, planning, housing, and medicine and health care. Fundamentally, the book examines the forces, innovations, and institutions that have shaped our urban society, provides a framework for understanding current urban problems, and advocates a variety of democratic planning tools to achieve humane and open urban communities.

It is impossible in a short review to do justice to the book's many and varied interpretive themes. One chapter elaborates the total planning of seventeenth-century New England towns; yet this "Puritan folk planning" was static in the face of a changing society, and orderly communities gradually eroded under the twin pressures of population growth and economic competition. A second chapter examines the American tradition of land usage, a tradition that viewed land as private property rather than a social resource. Thus, because of misplaced land-usage priorities, the urban grid-iron, the federal township system, zoning, city planning, and federal highway construction have all contributed to today's "disordered, inhumane, and restricted city." In another section, Warner establishes a rough chronological framework for understanding the development of the modern American city. For each of three fifty-year periods (1820-70, 1870-1920, and

1920-70), he shows how transportation and technology shaped the city's internal and external economy, its land uses, its business and corporate structure, its job and residential structure, and its human and societal problems. Archetypal cities (New York, Chicago, and Los Angeles, respectively) are used to illustrate the general patterns for each period. Additional chapters trace the emergence of what Warner calls the "urban cultural consensus" and the simultaneous persistence of class, racial, and socio-religious differences; the inadequacies of urban housing, both past and present; and the failure of health delivery institutions to provide effectively for the whole population.

Beyond these stimulating and suggestive "present-oriented historical essays," as Warner calls his chapters, the author also advocates a variety of "solutions" for present urban problems. These range from national, regional, and community planning to democratic socialism and include such proposals as the following: the elimination of racial and class segregation and discrimination; government entrance into the housing field on a large scale to guarantee decent housing for all Americans; transportation planning combined with a "full-employment, living-wage policy" that would permit every family to own a "nonpolluting" automobile (Warner opposes the investment of billions of dollars in new mass transit facilities, preferring utilization of the existing highway network instead); democratic control of the huge bureaucratized workplaces in which Americans work; and a health delivery system that remedies actual medical needs and provides preventive services as well. The American public, Warner contends, must demand "that government and business serve the goals of a humane society." Such proposals, of course, have been advocated before, but they seem all the more reasonable considered in a historical framework. Specific ways, however, of achieving these basic goals go unmentioned and will, one suspects, remain elusive.

The book is enhanced by more than one hundred photographs that illustrate key themes of the urban experience; more than just supplemental, they are an integral part of the text and buttress the writing in a significant way. *The Urban Wilderness*, in short, is one of the most important general studies in

urban history yet published—an imaginative use of history to foster understanding of the troubled, urbanized society in which we live.

Like Warner's wide-ranging survey, two of the three essays in the second book under review also afford penetrating insights into the history of urban America. In "The City as a Melting Pot," Arthur Mann reviews the three traditional theories of ethnic assimilation in the United States (melting pot, Anglo conformity, and cultural pluralism) and posits a new and perhaps more useful model for making sense of our "multi-ethnic" society. In "Four Stages of Cultural Growth: The American City," Neil Harris explores a neglected dimension of urban history and proposes a periodization for the development of urban culture. The third lecture, "An Urban Historian's Agenda for the Profession" by Sam Bass Warner, Jr., is a very personal document that describes the many-sided crisis confronting historians and suggests some alternatives. For different reasons, each of these volumes is worthy of the attention of historians concerned with the American city.

RAYMOND A. MOHL  
Florida Atlantic University

RICHARD SLOTKIN, *Regeneration through Violence: The Mythology of the American Frontier, 1600-1860*. Middletown, Conn.: Wesleyan University Press, 1973. Pp. viii, 670. \$25.00.

Since Henry Nash Smith's *Virgin Land* revealed the riches concealed in the mythology of the American frontier, a handful of literary scholars have explored aspects of the subject with impressive, if not spectacular, results. With the publication of this magisterial volume mythological investigation takes on a new dimension. Richard Slotkin, an assistant professor of English at Wesleyan University, has produced a book of monumental scholarship, enormous sophistication, and lasting significance. Few who read his 600-odd pages will dispute that he has convincingly demonstrated the influence of frontier mythology as a major force shaping the nation's literary outpourings between 1620 and the 1850s, or that the traditions evolved from the hunter-hero myth that he describes so expertly provide a structuring

metaphor for a study of the evolution of the national character.

His analysis of the nation's writing, both popular and literary, that substantiates these generalizations is terrifyingly thorough and utterly convincing. The Puritan's first contact with the New England wilderness, he shows, revealed especially in Thomas Morton's *The New English Canaan* and in the captivity narratives of the Pequot and King Philip's wars, made God, not man, the mythological hero, rescuing passive captives from the strokes of evil by grace, just as He regenerated their souls by salvation. By the end of the seventeenth century, as Indian conflict intensified, Puritan mythology revealed a lessening faith in righteousness as a tool for frontier conquest; man must debase himself to the level of his devil-like foes as he exorcised them from the forests, much as he might exorcise his own soul of sin. The wilderness was still a haunted land, but frontiersmen no longer dreaded its terrors so intensely as they mastered its mysteries.

Professor Slotkin sees a major turning point in mythogenesis with the 1716 publication of Benjamin Church's *Entertaining Passages Relating to King Philip's War*; for the first time Church made himself, not Providence, the hero of a captivity narrative, meeting his Indian foes as men, not demons. As other writers adopted this pattern a secularized view of the frontier emerged, based on rationalism and a deepening understanding, and with it a concept of the Indian as an idealized child of nature. Still another step toward the evolution of the hunter-hero was taken by authors who used Braddock's defeat to glorify American above British heroes. By the time of the Revolution, the ingredients essential for America's ideal mythological hero had been isolated.

The author who created this ultimate hunter-hero was John Filson when he added an appendix on "The Adventures of Col. Daniel Boone" to his 1784 edition of *The Discovery, Settlement and Present State of Kentucke*. Boone stepped from the pages of this fanciful tale as the archetypal frontier hero, the man who made the forest safe for democracy, concentrating within himself all the attributes of the hero-types that had emerged in colonial writing. From that time on the Boone myth was to serve as a barometer of the nation's cul-

tural progress, revealing in Boone's changing character alterations in the national consciousness. It even donned sectional garb, with New Englanders painting him as a debased Jacobin, Southerners as a disguised aristocrat, and Westerners as a realistic pioneer.

Against this background, Professor Slotkin argues, the Boone myth emerged during the 1840s as a distinctive literary form in the works of Cooper, Thoreau, and Melville. In the latter two particularly he finds the hunter-hero mythology generative, used in *Walden* to state in epic form the captive narrative in which the adventure impulse becomes a major force in the struggle to release the hero from forces imprisoning his mind and spirit, in *Moby Dick* to assemble the gallery of frontier myths with which Melville was familiar into a colossal hunt where all the elements of the frontier-hero myth are developed to their archetypal extremes.

No brief summary can do justice to the subtleties and complexity of Professor Slotkin's thesis; this is a book that should be read in its entirety by all with an interest in American writing, American violence, and the national character. The task is not to be undertaken lightly, for the author's multisyllabic vocabulary, sprinkled with the specialized terminology of the mythologist, and his leaden seriousness even when dealing with potentially humorous situations (he re-tells Thorpe's "Big Bear of Arkansas" tall-tale without cracking a smile) do not make for easy reading. Yet the reward is worth the effort. For those with the patience and skill to master the no-nonsense prose and thread their way among the bypaths that lead to a few dead ends, this is one of the few books that can be called definitive.

RAY ALLEN BILLINGTON  
*Huntington Library*

DAVID D. HALL. *The Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, 1972. Pp. xvi, 301. \$11.95.

Determining the effect of the New World on transplanted European institutions has long been a staple of American historical scholarship. In yet another work of the genre, David

D. Hall chronicles and interprets the development of the New England ministry from its English beginnings to the closing decade of the seventeenth century. His analysis deals with the birth and maturation of congregational patterns of doctrine and polity, the evolving relationship between Church and state, and the practice of evangelism among the clerics. The intent of the study is to discover how each of the first two generations of clergymen defined the role and status of the ministry and how their definitions mirrored the nature of the colonial experience.

The depth and richness of the written record left by the clergy in Massachusetts mean that the work is written largely from sources produced by Bay Colony clerics, but whenever possible material from churches in Connecticut is added to illuminate or strengthen interpretive assertions. His investigation also relies heavily on the writings of Continental and English clerics, incorporating material from theologians ranging in diversity from John Calvin to Edmund Calamy. Political, economic, and personal sources are ably employed along with the usual sermons and polemics to place the ministry in proper focus.

Although Hall concentrates on the evolution of colonial religion during the first twenty years of settlement, asserting that "within the context of Puritan history the change seemed predetermined," the progression he discovers is circular rather than linear, with the second generation of clerics forced by circumstances to modify their role and resurrect aspects of the rhetoric and doctrine their predecessors expounded in the first few years after their arrival in America. Within the compass of the thematic return to aspects of an earlier faith, several less vital issues are treated with considerable imagination. The conflict between advocates of an evangelical and a sacerdotal ministry, disputes over the nature of congregational church membership, and the problems of intrachurch membership are all interpreted in ways that might well cause revision of long-held conceptions on several aspects of New England's ecclesiastical history.

In any study that is dependent on discerning and evaluating causal factors, the essential task is to communicate the quality, intensity, and interrelationships of the factors in a manner

that is comprehensible and consistent. Hall is able to do this with skill. The conflicts between Church and state as well as disagreements between individual ministers and ministerial cliques are treated in a way that effectively communicates the preoccupations of the colonials to readers exclusively attuned to secular justifications for the use of power and the manipulation of men. But a certain measure of the empathy created with the clerics of three hundred years ago is achieved only by slighting the spiritual commitment that was central to the ministerial decision-making processes. The divines seem driven by considerations bound closely to authority, status, and security while matters of faith, duty, and a sense of devotion are included merely as peripheral items.

*The Faithful Shepherd* is a valuable addition to the historical literature of early America. Controversial interpretation is the stuff that induces more investigation, and surely by providing carefully constructed perspectives that will generate debate and give impetus to further research, Hall's work is an important step toward gaining a deeper understanding of the colonial period.

B. R. BURG

Arizona State University

ITALA VIVAN. *Caccia alle streghe nell'America puritana*. (Nuova collana.) [Milan:] Rizzoli. 1972. Pp. 751. L. 7,500.

Italian scholarship has recently made notable contributions both to colonial American history (Giorgio Spini's *Autobiografia della giovane America* in 1968) and to the history of witchcraft (Carlo Ginzburg's *I benandanti* in 1966); but Itala Vivan's attempt to combine these two specialties is a failure that will be useless to American readers. Her thick volume has been built from a wide range of scholarship, much of it excellent: her debts to G. L. Burr, Perry Miller, and Kai Erikson, for example, are considerable and fully acknowledged. But she never consulted the voluminous unpublished sources on her subject; her book acquires its bulk largely through copious translations of important published sources. The author is not so much a historian as a belletrist, as the number of entries under Nathaniel Hawthorne in the index testifies. And while

she is concerned to tell Italians something more about a period not well known to them, the first century of American history, she has chosen to tell it as a long tale of Puritan repression. There is an eighty-page excursus on "Preti feroci" cracking down on dissenters from Morton of Merry Mount through the Quakers; worse yet, there are several quick asides to Sacco-Vanzetti, the late Senator McCarthy, and even Bobby Seale.

The most important redeeming feature of this book is its useful stress on the wilderness and especially on the American Indians in shaping the special contours of magical and witchcraft beliefs in colonial America (pp. 12-13, 53-76, 95, 123, 133, 162, 222, 259, 344, for example); here is a major reason why witchcraft in New England was somewhat different from old England. But even its illuminating discussion of the "demonic" redskins is a small reward from a large book that all too often merely substitutes new clichés for old.

E. WILLIAM MONTER

Northwestern University

COTTON MATHER. *The Angel of Bethesda*. Edited, with introduction and notes, by GORDON W. JONES. [Worcester, Mass.:] American Antiquarian Society; Barre, Mass.: Barre Publishers. 1972. Pp. xl, 384. \$25.00.

In his own day Cotton Mather was accorded considerable status, but historians, physicians, and scientists in the nineteenth and early twentieth centuries relegated him to the dark ages. The era of debunking in the twentieth century, however, had a reverse effect upon Mather's image, and he has since emerged as a pioneer in medicine and science and a liberal in theology. If the process of brightening continues, it may well lead to a counter-reaction. Be that as it may, we now have the first complete edition of Mather's chief medical manuscript, "The Angel of Bethesda." Avid as he was for publication, it is surprising that he did not secure a publisher for it. The manuscript is long, however, some 410 pages or more, and it was not completed until the close of his life. By this time, too, he had already quarreled with most of Boston's physicians over the inoculation issue. For whatever reason, the manuscript languished for over two hundred years.

Mather's manuscript was dismissed by earlier

historians and lay readers largely because of its theological overtones. The manuscript is divided into sections, each of which deals with a specific disorder. In every case Mather begins with a short sermon in which he stresses the wrath and mercy of God and beseeches his readers to recognize their own unworthiness. Then follows a description of the ailment, and a list of the various therapeutics. Compared to the physicians of his day, Mather was surprisingly moderate in his prescriptions and demonstrated remarkable good sense. Yet he was a product of his times, and valid remedies are interspersed with calls for such items as crabs' eyes, warm urine and honey, sheep dung in white wine, and an infusion of hog dung and nettle-juice. In making these recommendations Mather was fully in accord with accepted medical practices.

Mather shrewdly observed that venereal disease was occasionally hidden under the diagnosis of phthisis (pulmonary tuberculosis), and he constantly urged his readers to avoid excesses—in drugging, diet, and bleeding. He was familiar with the current medical literature and well versed in the various medical theories. To his credit, he was one of the first to espouse the animalcular thesis, an early expression of the germ theory.

Dr. Gordon Jones has written a good introduction, carefully annotated the text, and made this rare medical treatise available. In so doing, he has contributed to our understanding both of Mather and his period.

JOHN DUFFY  
*University of Maryland*

J. A. LEO LEMAY. *Men of Letters in Colonial Maryland*. Knoxville: University of Tennessee Press, 1972. Pp. xvi, 407. \$13.95.

This permanently valuable book is diminished by summary, for its importance lies less in its argument than in its evidence. With the zealous detective work of older literary scholarship but using the latest scholarly resources, Lemay meticulously pieces together hundreds of unfamiliar bits of information that compose a new picture of early Southern writing. The combination of old and new is significant because *Men of Letters* is the first local pre-Civil War literary history to appear in several decades.

Just after the older scholarship succumbed to the New Criticism, technological helps like Xerox and microprint made possible extraordinary extensions of the scope and depth of early American literary history. But while much new material became accessible, the type of literary history that might have used it went out of fashion.

The chronological-biographical format of *Men of Letters* allows this accumulated material to be fully presented. Covering Maryland literary life from Andrew White to Thomas Bacon, the book draws on wills, college registers, land records, English magazines, and Maryland newspapers, to settle many long-elusive points of fact, attribute many anonymous poems, provide rich bibliographical notes to guide further research, and fill in many details about early Maryland publications, publishing, and writers. In sketching the lives of such mere presences as Dr. Alexander Hamilton and James Sterling—poets one has always known of, while knowing little about—Lemay adds a new dimension to one's mental picture of the colonial Southern literary situation. Ultimately the richness of evidence itself is a major argument: that belletristic activity in the early South was self-aware and extensive, enlivened by celebrities, gossip, clubs, a marketplace, and other features of metropolitan literary life.

Lemay's interpretations of his evidence are challengingly fresh and bound to provoke argument. Several attempts to relate Maryland literature to later, larger ideas in American culture rest on assertions that one writer or work "foreshadows" or "prefigures" or "anticipates" some later writer or theme—language that hints at influence where only similarity is evident. *The Sot-Weed Factor* becomes an archetypal frontier put-on, satirizing not Maryland but the narrator's view of it, and English notions about colonial life. If so, one wonders, is the narrator's disdain for Quakers also a satire on English views of Quakers? Lemay's answer is that Cook exaggerates some elements of Maryland life just enough to satirize Maryland; he exaggerates others so much that they satirize the people who would think them true. But does either category contain Cook's description of the plantation feast of "delicious Meats" and "Turkies wild Luxurious Chear"?

To raise such questions is to demonstrate the



value of this book to the study of early American literature. There was nothing before it to ask such questions of. By reconstructing a large and often skillful body of writing in the framework of the older literary history, Lemay creates a new field of inquiry. One must read his book to have an adequate picture of the colonial literary scene.

KENNETH SILVERMAN  
New York University

DAVID S. LOVEJOY. *The Glorious Revolution in America*. New York: Harper and Row, 1972. Pp. xvi, 396. \$15.00.

Nine years ago Michael Hall, Lawrence Leder, and Michael Kammen stimulated new interest in a comparative treatment of late seventeenth-century colonial politics with their collection of primary sources entitled *The Glorious Revolution in America*. Now in a similarly titled study David S. Lovejoy has provided a long-awaited assessment of those critical upheavals that disrupted the American colonies from Bacon's Rebellion in 1676 to the major revolts in New England, New York, and Maryland in 1689. His interpretation is decidedly neo-Whig, which should provoke a lively debate among scholars of the period.

It is Lovejoy's thesis that by the 1670s and 1680s, the colonists "had come to some hard conclusions about the English empire and the meaning of their life in America," and that they "reacted to events and ideas sometimes in a fashion similar to that of their grand and great grandchildren in the 1760's and 1770's" (pp. xv, xvi). These hard conclusions were the result of a keen awareness and defense of individual and charter rights under assault by an intruding imperial bureaucracy. Lovejoy graphically describes England's new assertive role in commercial, political, and religious aspects of colonial life following the Restoration. Then, proceeding colony by colony, he recounts the conflicts over sovereignty, the application of English laws, legislative privileges, and individual liberties. He discusses with admiration the assembly debates, petitions, and grievances upon which he rests much of his argument for the ideological origins of the Glorious Revolution in America. To borrow Thomas J. Wertenbaker's phrase, Lovejoy por-

trays these rebels as torchbearers of the American Revolution.

This book properly reminds us of the widespread discussion of political rights and liberties in late seventeenth-century America, but it prompts many disturbing questions as well. Does Lovejoy not perhaps overemphasize the role of ideas with insufficient attention to the social bases of these revolutions? Were not these turmoils also power struggles over who should rule at home? Although the author observes the presence of factions, any internal divisions appear clearly less significant to him than the collective colonial response he detects to the new English imperial system. Finally, how did the majority of colonists, who were not directly involved, perceive these struggles, and what was the impact of the disturbances on society at large?

Most of the secondary literature on these revolutions fails to address these concerns satisfactorily; that literature remains quite uneven, largely narrative in nature, and generally restricted to a discussion of political events on the provincial level. For this comparative synthesis, Lovejoy has had to rely very heavily on his own extensive research, but this too has been largely in those published primary materials and official provincial records that afford little information for a broader social analysis of the period. It has been a considerable task simply to establish a reasonable chronology and account of the uprisings, which the author has done very well in this straightforward political history with its primary attention to the articulated issues of contention.

Answers to the troublesome questions and a testing of Lovejoy's emphasis on an ideological struggle may rest in the fragmented, widely scattered, and largely unpublished local records that historians are mining so profitably for their community and demographic studies. Such records for Maryland would suggest some important modifications of Lovejoy's thesis at least for that colony, where in 1689 rebels turned toward, not from, English rule in their revolt against Lord Baltimore. Men united against the proprietor in the political struggles of the 1670s and 1680s bitterly opposed each other in the final revolution. Career profiles of both factions suggest that significant social and political characteristics, not primarily positions

on political principles, distinguished the two groups and helped to influence their alignment. These fragmented county records do, however, support Lovejoy's picture of a more politically sophisticated and mature society than is usually assumed. Although the revolution rendered the colony without normal provincial government for three years, county institutions were little affected as opposing parties cooperated sufficiently at that level to ensure basic stability and continuation of both essential and routine political activities.

Maryland may be the exception, but much recent literature has demonstrated the importance of social analysis and the value of further research in the largely untapped local records. More intensive studies of these revolutions are still needed. Meanwhile, Lovejoy has provided a fine narrative of the period and a most provocative comparison of these important revolutions, a comparison that should challenge all students of the colonial political process.

DAVID W. JORDAN  
Grinnell College

IRA ROSENWAIKE. *Population History of New York City. (A New York State Study.)* [Syracuse:] Syracuse University Press, 1972. Pp. xvii, 224. \$12.50.

The main concern of this book is to present "the statistical dimensions of three centuries of change" in the population of New York City as they appear in the unusual number of censuses available for this town. In practical terms this means a focus on the actual size of the population of the city at various points in time as well as on the number of persons of different racial, ethnic, and religious backgrounds who inhabited New York throughout its history. In addition the author explores the reasons for the growth of population. He emphasizes the role of migration, not only because the movement of people in and out of the city was the main cause of its demographic change, but also because evidence on the effects of natural increase (the excess of births over deaths) is somewhat limited for the period before 1900.

Few historians will be surprised by the conclusion that one ethnic or racial group has continually been replaced by another throughout New York's past. Students of urban history,

however, will appreciate the detailed documentation of the rates and patterns of turnover. Among the more specific findings that should be of interest are the extraordinary loss of native-born Americans to the rest of the United States, the extent of the movement to the suburbs that began about 1830, and the fact that poverty and ghettoization were not universally associated with high birth and death rates.

In the end, however, this book may be of restricted value to many historians because of its narrow conception of the limits of demographic history. Surely some discussion of the age and sex structure of the city's population could have been added to the comments on size and ethnic and racial composition. Rosenwaike notes New York's economic attractiveness to immigrants. Yet he ignores the possibility that, by bringing in unusually large numbers of adult males, migration may have been as much a cause as an effect of economic development.

The book has merit. The author has done well what he set out to do. But it seems clear that if demographic history is to hold an interest for many historians, future work on New York (and other cities) must ask other questions of the censuses (as Thernstrom and others have done), and it must exploit more fully the noncensus sources that shed light on population change. Only with the most narrow view of demographic history could one claim, as the dustcover does, that this is "the definitive reference work on the demographic history of our nation's largest city."

ROBERT V. WELLS  
Union College

MAXWELL WHITEMAN. *Copper for America: The Hendricks Family and a National Industry, 1755-1939.* New Brunswick: Rutgers University Press, 1971. Pp. viii, 353. \$12.50.

Mr. Whiteman traces the business and personal history of the Hendricks family through four stages of the American nonferrous metals industry: the colonial and early national periods, when a vast carrying trade brought most of America's copper, tin, spelter, lead, and antimony from England; a period during and following the War of 1812, when South American imports were substituted; the years of uninterrupted search for native American ores and the

development of mills along the Eastern seaboard; and, finally, the exploitation of the copper deposits of the Great Lakes region and the gradual westward shift of processing and manufacturing facilities after 1840. In each stage members of the Hendricks family were leaders of the industry; firms bearing their name were active in the copper trade as late as 1939.

The bulk of Mr. Whiteman's narrative relates to the years 1755-1869, to the mercantile house founded in New York by Uriah (1737-98), and to the Soho Copper Works founded in Belleville, New Jersey, by his son Harmon (1771-1838) and carried on by Uriah II (1802-69). Extensive information is provided on the mechanics of the import and inland trades that supplied the raw-material requirements of America's coppersmiths, brassfounders, tin-smiths, plumbers, and pewterers (including such notable Hendricks customers as Paul Revere, Robert Fulton, and James P. Allaire). Similarly, the corporate history of the first successful copper-rolling mill in the United States and the rise and fall of a market for American-made copper ship-sheathing and locomotive and boiler parts are fully covered. As merchants and manufacturers the Hendrickses were nonferrous metals specialists and are important as pioneers in that industry; they do not, however, seem to have been innovators in general business administration.

In addition to these business details, the social life of the family shares center stage. Upon his arrival from London in 1755 the first Uriah associated himself with New York's Spanish-Portuguese congregation, Shearith Israel. In 1762 he married into the prominent Gomez family. Thus identified with the earliest Jewish settlers of New York City and affiliated with the same synagogue for over two hundred years, the Hendrickses played active roles in the cultural and religious life of New York's Sephardic community. And because kinship and business were so intertwined in this community, Mr. Whiteman's careful unraveling of genealogical ties is most welcome.

It would appear that the large Hendricks family collection in the New-York Historical Society is a treasure trove of early American industrial and religious history. Mr. Whiteman's organization and use of these materials is a good first step toward their full exploitation by

business, technological, and cultural historians.

JAMES P. BAUGHMAN  
Harvard University

CLIFFORD K. SHIPTON, *Biographical Sketches of Those Who Attended Harvard College in the Classes 1764-1767*. (Sibley's Harvard Graduates, volume 16.) Boston: Massachusetts Historical Society. 1972. Pp. 598. \$20.00.

Dr. Shipton begins volume 16 of Sibley's *Harvard Graduates* with this statement: "After forty years and thirteen volumes there is nothing new for the editor of this series to say by way of introduction." But being a pastmaster in the art of the biographical sketch, he has a wealth of information, entertainingly written, to contribute to the historian and the genealogist. The four Harvard classes in this volume totaled 197 young men, a cross-section of the professional and business communities of New England, with a few other occupations (farmer, surveyor, tavernkeeper) for good measure. Except for four students from other British colonies and two from Europe, they were natives of New England, mostly of Massachusetts, thus attesting to the provincialism of Harvard College in the pre-Revolutionary period.

These sketches of Harvard graduates reveal very little about them individually as undergraduates, for the operations of the college were officially recorded with a formality and brevity that tantalize the historian; collateral evidence is also sparse. Nevertheless, the repetitious nature of the data has its peculiar value, supplied in quantity, whether the information pertains to roommates or social status, to theses debated or disciplinary action taken, to student pranks or student membership in the Association for Suppression of Vice. Dr. Shipton's sensitivity for the apt quotation is well illustrated by the Butter Rebellion of October 1766, spearheaded by Daniel Johnson, class of '67, who, summoned before the faculty, stated "that the only Reason of their going into those Measures, was to procure *better Butter*."

One-third of the graduates became ministers of the gospel. By this period the occupational range of Harvard men was widening: 20 per cent became merchants, 16 per cent physicians, 12 per cent lawyers, and 11 per cent schoolmasters: there were a few unfortunates by reason of instability, changing times, or just hard

luck. The Revolutionary crisis, which began to quicken within a few years of their graduation, lends interest to the careers of these New Englanders, 24 of whom served in the American armed forces and 32 of whom became loyalists. Although only a few won reputations beyond their own region (e.g., Manasseh Cutler of the Ohio Company and Jonathan Loring Austin of the secret foreign service for the Continental Congress during the Revolution), among the total lot were two governors of Massachusetts (Caleb Strong and Increase Sumner), a noted educator (the Reverend Enos Hitchcock), and a bishop of Massachusetts (the Right Reverend Samuel Parker). Most picturesque was the French physician, Dr. Peter De Sales Latrrière, whose romantic and adventurous life has the makings of a fascinating historical novel. Some of these worthies appear among the thirty-one illustrations in the book.

The historian of eighteenth-century America should not overlook the rich crumbs of evidence on many aspects of life of this period, personal and social, interspersed throughout the biographies. Here is an excellent case of the whole being greater than the sum of its parts. The only shortcoming is the index, which focuses too exclusively on personal names and places of birth and residence of the graduates. Those rich crumbs are hidden among the 550 pages.

LESTER J. CAPPON

*Atlas of Early American History*

DONALD L. ROBINSON, *Slavery in the Structure of American Politics, 1765-1820*. (The Founding of the American Republic.) New York: Harcourt Brace Jovanovich, 1971. Pp. xii, 564. \$13.95.

The author, a political science professor, describes his book of eleven chapters and lengthy "Notes" as "a study of the impact of slavery on the founding of the United States as a national political community" (p. 3). In excellent detail he presents developments from the beginnings of slavery in English America through conventional periods such as the American Revolution, the Confederation, the Philadelphia Constitutional Convention, and the early presidential administrations. There are two chapters each for the Revolutionary era and the Constitutional Convention, another on "The At-

tempt to End the Importation of Slaves," and one in which the central concerns are the Northwest Ordinance and the Missouri Compromise. In the area of foreign affairs the chief topics are Jay's Treaty and the revolt in Santo Domingo led by Toussaint L'Ouverture.

Professor Robinson's book bears the marks of careful scholarship. Although he stresses that "slavery and racial prejudice" were "seeds of the sectional clash of 1860" that "were in the soil at the nation's founding" (p. 444), it is doubtful that any close students of American history will find any of his information new or startling. Most of his interpretations have long been available, especially in the works by such black historians as George Washington Williams, W. E. B. Du Bois, Carter G. Woodson, Lorenzo Greene, Benjamin Quarles, John Hope Franklin, and others.

Two of Robinson's statements on the Constitutional Convention are revealing. He writes: "The careless language of the fugitive slave clause reveals better than anything else in the Constitution that the fundamental problem for blacks in the union was that . . . white Americans in general did not hold them in just regard" (p. 230). He makes it clear, once again, that, in the areas of both domestic and international relations, slavery was one significant aspect of the Federalist-Antifederalist divisions.

At times the discussion of slavery is lost sight of, in the interest of that other part of the book's title, the structure of American politics. Although everywhere the plight and activities of free blacks were intimately tied to that of their brethren in bondage, free blacks are practically never mentioned. Probably of greater importance, however, is that here, once again, the voice of the slave is silent. No evidence is presented that any of the slave narratives were consulted. Although the author's excellent "Notes" (pp. 451-543) indicate familiarity with numerous sources that reveal the impact of the thought and deliberate acts of blacks, use of this material is not reflected in the text. Robinson points out that his central concern is "to record and to judge the ways in which the founders of the American political system dealt with" the institution of slavery, yet at another place he indicates that his goal is the larger one of "writing something about the black presence and its significance during the found-

ing of the American political system" (p. xi, xii).

Another possible defect of this book—but by no means peculiar to it—is that Robinson writes as though the defense or toleration of slavery by many white Americans was due solely to rational factors. It may be time for all scholars to consider whether the Founding Fathers and their immediate successors as national leaders were victims of such irrational forces as Joel Kovel points to in his book, *White Racism: A Psychohistory* (1970).

Robinson points out that, except on the subject of the slave trade, at the Constitutional Convention the South held to "absolute intransigence on the subject of power to abolish or regulate slavery" (p. 211). Because of such things as greed for profits, power, and social prestige, too few persons were willing to listen to James Madison's lament that the "distinction of color [had been] made, in the most enlightened period of time, a ground of the most oppressive dominion ever exercised by man over man." Many black Americans still lament that almost two centuries of enlightenment since 1787 have not been enough to end all of the "oppressive dominion."

EARL E. THORPE

North Carolina Central University

LETITIA WOODS BROWN. *Free Negroes in the District of Columbia, 1790-1846*. (The Urban Life in America Series.) New York: Oxford University Press. 1972. Pp. ix, 226. \$7.95.

Letitia Brown's meticulous study of the laws and customs governing slavery and manumission and the treatment accorded free Negroes in the District of Columbia before 1846 throws new light on the reasons for the steady growth of a self-respecting community of freedmen and a corresponding shrinkage in the percentage of slaves. Mrs. Brown discusses several circumstances that encouraged this development in the area carved out from the two states with the heaviest concentration of Negroes in the country in 1790: the ideological climate of the region in the post-Revolutionary period when belief in the rights of all men still ran strong in Virginia and Maryland; the "fluid quality" of the social structure taking form in the new

capital city; the "confusion, the loopholes, the oversights in the law, the scattered, episodic, uncoordinated physical development of the Federal facilities"; and the "good fortune" of having "in positions where actions could implement ideas" men who were sympathetic to Negro freedom (Massachusetts-bred Judge Cranch, for example, who presided over the Circuit Court of the District of Columbia for fifty years).

The volume of litigation initiated by slaves to establish their claims to freedom and by freedmen to defend other rights may well astonish readers familiar with the curbs imposed on Negroes by neighboring states in 1806 and after. In the District, unlike Virginia, manumission was never restricted or conditional on the freedman's leaving the jurisdiction within twelve months, and, unlike Maryland, no municipal ordinances or congressional acts barred incoming free Negroes from obtaining permanent residence.

Two appendixes supplement the discussion of free blacks' material progress, one listing their occupations in 1835 before a race riot led the city to limit them largely to menial jobs, the second naming Negro property owners and the assessments on their holdings at five dates between 1824 and 1846. Although, as the final chapter notes, "In many ways Washington crystallized into a Southern city as the Civil War neared, . . . elements of its earlier heritage and unique function persisted. . . . The special urban setting in the District with its tradition of contractual labor . . . continued to encourage enterprise and to supply enough employment for subsistence." If in this and other chapters the text seems excessively detailed at times, the preciseness of the resulting information is well worth some extra verbiage.

CONSTANCE MCLAUGHLIN GREEN

Washington, D. C.

JAMES THOMAS FLEXNER. *George Washington: Anguish and Farewell (1793-1799)*. Boston: Little, Brown and Company. 1972. Pp. xii, 554. \$15.00.

In this final volume Flexner is so immersed in Washington that at times he seems to be standing beside the man and reading his inmost



thoughts. Thus the account of Washington's death reads as if Flexner had been in the room at the time. Flexner's story is that of an aging man whose memory was failing and who made political mistakes that he would not have made when younger. The nadir was reached in 1798 when President John Adams appointed Washington to create an army in case of war with France. The quarrel over the appointment of subordinate generals leads Flexner to the sad conclusion that Washington blew up a "medium sized disagreement, into an issue that threatened to convulse the nation. And at various moments in the controversy the brilliant pragmatist seems to have lost contact with reality. . . . Not even the most resplendent hero is immune to the passing years."

There are numerous errors: John Beckley was clerk of the House of Representatives, not a congressman; Samuel Chase, not Thomas Chase, was appointed to the Supreme Court; state legislatures elected United States senators in all the states, not in "most states"; the "radicals" did not "often" create unicameral legislatures but created only two: Pennsylvania and Georgia.

There are good things, however. One is the account of Washington's desire to free his slaves. Another describes his efforts to sell some household goods and two horses to the incoming president, John Adams. Adams was a farm boy, too, and he had no intention of paying £1,000 for what he said were old horses. Flexner's account of Gilbert Stuart's portraits of Washington and of Washington as an art collector is fascinating. While Washington bought prints of the usual semidraped nymphs, he really wanted "realistic" paintings of the American landscape. Flexner's conclusion that Washington was the grandfather or great-grandfather of the Hudson River school of painters will intrigue some readers and startle others.

Thus in this, as in the previous volumes, one will find much of interest and value, but one will not find an adequate discussion of the public issues with which Washington was involved, or even many clear statements as to what those issues were. But this is not a history of the times. It is the history of a man written with sympathy and an undeviating belief in

Washington's greatness, even when he slips from the high pinnacle upon which the author has placed him.

MERRILL JENSEN  
University of Wisconsin,  
Madison

ELWYN A. SMITH, *Religious Liberty in the United States: The Development of Church-State Thought since the Revolutionary Era*. Philadelphia: Fortress Press, 1972. Pp. xiv, 386. \$10.95.

The first two parts of this volume constitute a major contribution to American religious and intellectual history. Perhaps for the first time one can find between the covers of a single book powerful reinterpretations of the evidence surrounding a theme that was central to both Protestant and Catholic experience. Professor Smith's clarification in the first section of what he calls "the separatist tradition" of Church-state thought in the new nation rests upon his solid grounding in Reformed theology. He makes plain, for example, the significance of both natural law and eschatology in the ideas of Isaac Backus, and shows far more clearly than Sidney Mead or Wesley Gewehr did the interplay between popular Presbyterianism and the republican philosophy of James Madison. His powerful chapter on "The Moral Government of God" displays a familiarity with both the details of theological speculation in New England and the experiences of pastors who tried to bring that speculation to bear upon their immediate challenges. The discussion of the mid-nineteenth-century controversy between Protestants and Roman Catholics explains and to some extent justifies the Protestant point of view, thus preparing the reader to consider seriously the Catholic arguments set forth in the second part of the volume.

The six chapters dealing with the Catholic tradition, from John Carroll to John Courtney Murray, are somewhat more diffuse. Bishop John England, of Charleston, South Carolina, receives the thoughtful appreciation that is his due. With judicious borrowings from Thomas T. McAvoy, Smith is able in two chapters to make crystal clear the issues in public aid to parochial education that McAvoy, Robert Cross, and others have left unclear. He relies

too heavily, I think, upon Ray Allen Billington's work on mid-nineteenth-century nativism and neglects Vincent Lannie's study of Archbishop John Hughes's role in the school controversy.

In the final section Professor Smith provides a reasonably lucid account of the arguments of lawyers and Supreme Court justices dealing with Church-state issues, especially in the twentieth century. The style and substance of this section is more technical and theoretical than the earlier parts of the book, but its clarification of precisely how and why the metaphor of a "wall of separation" between Church and state in America has repeatedly confused lawyers as well as the public is a splendid achievement. I, for one, believe the links between the "constitutional tradition" Smith describes here and the various religious traditions will become clearer when someone produces an equally careful study of the attitudes and beliefs of American Jews.

The three sections constitute, to some degree, separate narratives of the history of Church-state thought, each overlapping the other in time. One may question this organization on the grounds that no one of these traditions was in fact at any given period developing without continuous dialogue with the others. The task of making clear what has been an impenetrably foggy historical swamp was the primary one, however; and if a choice had to be made, Professor Smith was wise to make the one he did, in preference to the pursuit of a reconstruction of the interplay of the various traditions in each period. He has managed to give to both the political theory and the history of American religion a clarifying set of insights.

TIMOTHY L. SMITH  
*Johns Hopkins University*

GEORGE D. GREEN. *Finance and Economic Development in the Old South: Louisiana Banking, 1804-1861*. Stanford: Stanford University Press, 1972. Pp. xiii, 268. \$8.75.

To relate the institutional history of banking to economic development is an exacting task, requiring the historian to grapple with economic theory as well as to confront more familiar problems of data and interpretation. But to write banking history of the antebellum

United States is especially difficult because of the extreme decentralization of governmental authority over banking which prevailed in that era. Each state had its own peculiar (usually changing) policy "mix" ranging from outright prohibition or tight regulation of banking firms to permissive, or promotional, measures that encouraged proliferation of banks and laxity of operational practices. Moreover, both the First and the Second Bank of the United States lent additional complexity to the institutional framework and the dynamics of banking's impact on the direction and pace of economic change.

Professor Green, trained in both economics and history, uses his knowledge of the two disciplines to good effect in this case study of banking policy, practice, and economic impact in one state. After summarizing the principal economic theories that are relevant to an understanding of how banking interacts with the process of economic change, Green offers a summary account (cast in a matrix of business-cycle history) of how Louisiana banks functioned in mobilizing capital, providing a paper money supply to the economy, extending short-term and intermediate-term commercial credit, and providing long-term loans on the collateral of real property. He relates the development of banking to the state's public finance, provides full analysis of the legislative history of major legislation of 1842 and 1853, and, more generally, seeks to delineate the "ties to the outside world" that linked Louisiana banking (and the commerce that flowed through New Orleans) with the mercantile and financial institutions of the Anglo-American trading network.

The most original and striking feature of Green's study is the explicit effort to evaluate the rationality of Louisiana's public banking policies and bank practices in light of economic theory. Although he encounters difficult data problems, some of them quite unsatisfactorily resolved (e.g., consider the slender basis for estimating "income from Louisiana commerce," omitting even income from marketing imports, at page 197), one must welcome his efforts to develop a one-state statistical series that can aid in assessing the central problem of the book—the impact of banking on development. His main conclusions are that in the 1820s and

1830s financial intermediation by banks "accelerated economic growth" (p. 2), that from 1840 to the early fifties restrictive public policies hampered Louisiana's growth, and that after 1853 more expansive banking once again served as a stimulus to the "real economy" (pp. 4, 56, 163 ff.).

Green argues, too, that neither banking nor perpetuation of the slave-labor system substantially diverted Louisiana from the path of optimal development, in the long run, based on regional comparative advantage. But here he falls prey to the tempting prospect of assaulting historiographical straw men, taking aim at the "primary emphasis" of works that do not pretend to take adequate account of how Louisiana, with its great New Orleans entrepôt, deviated from the "Southern" norm. He also fails sometimes to deliver what is implicitly promised: for example, to demonstrate that the decline of the relative trade position of New Orleans can be explained rigorously by reference to inadequate credit facilities as well as to the broad impact of the transportation revolution; or, that Richard Easterlin's data and hypotheses can be interpreted quite differently than is customary (see the logical cul-de-sac at page 179, note 20).

However that may be, scholars are indebted to Professor Green for a bold, explicit introduction to the interpretive issues in the light of relevant theory, and for a thoroughly researched, thoughtful, and well-written case study of one state's banking system.

HARRY N. SCHEIBER  
*University of California,  
San Diego*

CARROLL SMITH ROSENBERG, *Religion and the Rise of the American City: The New York City Mission Movement, 1812-1870*. Ithaca: Cornell University Press, 1971. Pp. x, 300. \$10.50.

NATHAN IRVIN HUGGINS, *Protestants against Poverty: Boston's Charities, 1870-1900*. Foreword by OSCAR HANDLIN. (Contributions in American History, number 9.) Westport, Conn.: Greenwood Publishing Corporation, 1971. Pp. xiv, 225. \$10.00.

ALVIN W. SKARDON, *Church Leader in the Cities: William Augustus Muhlenberg*. Philadelphia: University of Pennsylvania Press, 1971. Pp. 343. \$15.00.

PAUL A. CARTER, *The Spiritual Crisis of the Gilded Age*. DeKalb: Northern Illinois University Press, 1971. Pp. xiii, 295. \$8.50.

The chief danger of a lengthy review that deals with more than one book is that it will to an inordinate degree express the reviewer's opinion on a general unifying topic and neglect the books under review. This threat becomes especially strong when, as in this case, the editor of the *AHR* has assembled a group of books whose interrelationships are both numerous and significant. But I intend to restrain myself.

First to be discussed are three excellent monographs, all of which are revised doctoral dissertations that demonstrate again the invaluable nature of such publications. I feel no inclination to make the familiar kind of condescending remarks, because each of these works exhibits the results of thorough research, careful documentation, thoughtful generalization, and felicity of style to a greater degree than many supposedly more mature monographs. Each of them makes substantial contributions to historical knowledge in an area of great importance to our understanding of the vast social and intellectual problems that beset nineteenth-century America. May their numbers increase!

Carroll Rosenberg's study of the New York City Mission movement and its impact on charitable activities provides an important new approach to the process by which socially influential New Yorkers became aware of poverty as a major problem and then organized themselves to do something about it. She describes how older views were transformed and new kinds of energy unleashed by the great revivals of Charles G. Finney and how this in turn led to city-wide evangelistic campaigns. Her intensive studies of the New York Tract Society and over a score of later charitable organizations reveal how efforts to reach all New Yorkers gradually led many earnest men and women to a discovery of the desperate condition of the poor and then increasingly to go beyond the distribution of tracts and Bibles to the offering of material assistance. As awareness of these needs deepened, less specifically evangelistic agencies were founded, but into these as well the pious aims of revivals continued to operate. As she brings the narrative down to 1870 one learns of an entire era in social reform, the emergence of pro-

fessional forms of social work, a growing realization of the importance of a blighted environment, and also the rise of serious efforts to enlist the legislative power of government, especially in the improvement of housing. In the end one is led to concur with her assertion that the earlier period was "as much the Age of Finney as that of Jackson" as well as with her larger conclusion that the Second Great Awakening had a powerful animating effect on New York's charities and that religious attitudes continued to be important even after the Civil War, when secular tendencies of many sorts were more pronounced. The author's precision of statement, use of evidence, and penetrating interpretations make the book an important contribution to a little-studied aspect of New York's history.

Nathan I. Huggins's carefully researched and sharply reasoned account of the essentially Protestant philanthropic agencies and the social and moral attitudes that dominated Boston's organized response to poverty appears now almost as if it were designed as a sequel to Carroll Rosenberg's work. Not only does he study the next half century, in which the "Charity Organization Movement" took over the less coordinated efforts of the antebellum period, but he follows up important themes developed in the New York study. Among these are the tendencies to professionalization and secularism that were so pronounced in Boston. Huggins, in fact, emphasizes the degree to which warmly Christian motivation that animated the advocacy of Channing and the pioneering labors of Joseph Tuckerman became almost imperceptible in a theorist like Edward Everett Hale. In contrast to the situation in New York and many other parts of the country, moreover, one cannot but note the relatively small influence in Boston of evangelists like Finney and the overwhelming importance there of the Unitarian establishment. The basic task, of course, for Huggins as for Rosenberg, was to provide a basic account of the way new agencies were founded and how they adapted themselves to increasingly heavy burdens. And in the 1870-1920 period, when governmental aid was still insignificant, this meant the development of greater efficiency and hence much closer cooperation. Be that as it may, Huggins's interpretation of changing attitudes toward the

poor as the problems of urban poverty became more serious and complex are unusually discerning.

Pervading Huggins's entire account, however, is another theme, which Rosenberg necessarily also dealt with, the immense degree to which all of the humanitarian endeavors in both cities were conditioned by a settled conviction that their effectiveness depended on a distinction between the worthy and the unworthy poor, a view that grew sharper and more culturally oppressive with the passage of the decades as problems of urban poverty deepened and as the poor came to be of non-Protestant background. Huggins by no means ignores organizational history, nor does he unfairly demean the motives and achievements of many earnest men and women in the movement. Indeed his intellectual history of the entire philanthropic impulse is remarkably sensitive. Yet his whole discussion is pointed to an observation in his epilogue that has a very contemporary ring. "The reformers," he says, "were unable to see a simple truth that has struck some present-day social thinkers: the problem of the poor is that they have no money."

Skarden's biography of William Augustus Muhlenberg (1796-1877) provides an important account of how one enormously active church leader with deep humanitarian commitments and even deeper concern for Christian nurture and the life of the Protestant Episcopal Church wrestled with the intellectual and theological tensions of his day and at the same time struggled (within the limits of his ecclesiology) to make the Church itself an instrument of social amelioration. Muhlenberg's youth was spent in Philadelphia, but after a brief pastorate in Lancaster he came to the New York area in 1827 and remained there until his death. During this half century he had two major careers, first as founder and head of Flushing Institute, a preparatory school for the sons of the rich that became a model for many other such institutions, a number of them founded under Episcopal auspices. After 1845, however, as rector of the Church of the Holy Communion in lower Manhattan, his outlook became considerably enlarged. He made his a "freechurch" in which the owners of pews would not hold inordinate power, and he even withheld his parish from membership

in the diocesan convention wherein pew holders were dominant. He also made his church a center of devout worship and liturgical innovation despite his strong antipathy for the Oxford Movement. In this same spirit he sought unsuccessfully to formulate a compromise between the Catholic and Evangelical wings of the Episcopal Church. With regard to specifically urban problems he is remembered for several efforts: the founding of an infirmary and tenement building and then of St. Luke's Hospital. As a means of staffing these and other similar institutions he founded a sisterhood of deaconesses "for ladies of quality who have passed the first bloom of life and are still single."

Skardon's biography goes far beyond existing scholarship on Muhlenberg's eventful career. Heretofore our knowledge has depended almost entirely on the adulatory *Life and Work* (1880), which Muhlenberg himself committed to his chief coworker, Sister Anne Ayres, with instructions that she thereafter destroy all of his personal papers. Since she did both tasks with extreme thoroughness, it took nearly a century and extremely zealous research efforts to produce a biography that would significantly enlarge our knowledge of Muhlenberg's many-sided career. But that time, fortunately, is now come.

As one moves in this review toward a consideration of the "spiritual crisis of the Gilded Age," it is natural to wonder how Muhlenberg responded to the many new intellectual dilemmas that pressed upon his later years. These, of course, were questions with which Anne Ayres could not effectively deal, but even with the flood of new information that Skardon brings one must say that Muhlenberg brought relatively timorous succor to the troubles of the age—whether on Reconstruction in the South, poverty, Biblical criticism, or Darwinism. Yet his constant wrestling with the issues reveals their depth and range and underlines the need for a comprehensive view of religious problems in postbellum America.

Paul A. Carter's work on the spiritual crisis of the Gilded Age, unlike the works so far considered, is not, strictly speaking, a monograph; nor is it a historical survey of the period. His approach is basically topical, with each of his ten chapters dealing in one way or another with

the religious, moral, and social aspects of the many problems of the age. One chapter uses Henry Ward Beecher's sensational career as a window on the times. Another uses the writings of Lew (Ben-Hur) Wallace, Ambrose Bierce, Elizabeth Phelps Stuart, and Margaret Deland to illuminate various crosscurrents of doubt and conviction. Other chapters tell of encounters with Darwinism, Biblical criticism, Eastern religion, racism, slums, immigration, the social Gospel, Catholic-Protestant conflict, and the new type of urban revivalism. Over two dozen drawings and cartoons of the day and very frequent allusions to or comparisons with dilemmas of the later twentieth century add liveliness to the account. The author's animated style as well as his well-selected quotations also heighten the reader's interest in the period. Over forty pages of highly substantive annotation make fully manifest the author's long and deep immersion in this field, though unfortunately they are not placed on the page where they could easily be read in context.

Taken as a whole the book is a serious and valuable contribution to our understanding of the period in American history that still remains less adequately studied and interpreted than any other. One of the strongest features of Carter's study is his clear depiction of crisis—the depth of the anxiety of the age, the diverse questions faced, and the way in which many of the age's answers adumbrate those twentieth-century problems that he treated in his earlier book on *The Decline and Revival of the Social Gospel* (1954). The chief shortcoming of the work is that its emphasis on the texture and color of the age (often revealed through minor writers) precludes detailed exposition of more eminent thinkers, such as Josiah Royce, William James, Borden P. Bowne, W. G. Sumner, Albion Small, and Henry George. Yet this work fills an important need, and it would serve excellently as a college text on a vital period of transition if it were issued in a paper edition.

As I stood back from the century of rampant growth, drastic social change, and deepening crisis that each of these books has in its way illuminated, I was tempted to state a considerable number of impressions that have had their validity strengthened. Yet I will restrict myself to two. One pertains to the continuing hold of



Protestant convictions and values despite powerful pressures of secularization. The other has to do with the remarkable degree to which the changes in Protestant thinking seemed to intensify rather than weaken the ways in which the Protestant ethic encouraged the growth of an unregulated capitalistic society. Both of these considerations, not surprisingly, emphasize the importance of the social as well as the religious aspects of history that each of these four authors has been pursuing.

SYDNEY E. AHLSTROM  
Yale University

JAMES P. BAUGHMAN. *The Mallorys of Mystic: Six Generations in American Maritime Enterprise*. (The American Maritime Library, volume 4.) Middletown, Conn.: Wesleyan University Press for the Marine Historical Association. 1972. Pp. xviii, 496. \$17.50.

James P. Baughman has reminded us that behind the stark trend lines that measure the patterns of economic development in nineteenth-century America there were real men and women doing things, making choices and money, building homes as well as firms, sometimes going bankrupt, and occasionally founding financial dynasties. These people actually had children who were more important to them than one could tell by analyzing demographic data, and the children and their parents sometimes disagreed even when they were all attempting to maximize profits in a common business venture.

In this case the family is the Mallorys and their business history began in the years following the War of 1812, when Charles Mallory, a sailmaker in Mystic, Connecticut, used the money and knowledge he obtained from his craft to become a maritime capitalist. He first took shares in other men's whaling voyages and then became a managing agent for various whalers; meanwhile, he had begun to invest in the carrying trade and then during the forties became a shipbuilder. In the following decade he wisely shifted his money out of whaling, concentrating instead on ship construction, coastal shipping, and finally commercial banking. By 1860 he had turned an initial investment of \$1.25 into a fortune of \$300,000—a success story that reminds us that while Americans

may not have been as upwardly mobile as they liked to think they were, some of them did manage to improve their situation slightly in the antebellum years.

The subsequent generations of Mallorys deployed and increased this fortune in a variety of maritime companies. They ran commission houses, guided the affairs of the New York and Texas Steamship Company, played a role in a twentieth-century nautical combine—the Atlantic, Gulf and West Indies Steamship Lines—and even provided some second-level leadership for the United States Shipping Board in the First World War. During the twenties and thirties, Clifford Mallory carried forward the family tradition by conducting a coastal and international shipping business, first under government auspices (1919–25) and then as an unsubsidized, independent operator. Baughman ends his saga in 1941, when the death of Clifford Mallory ended the family's involvement in this last seaborne business.

In recording the exploits of the Mallorys the author does not always avoid the sand bars that have grounded lesser business histories. At times he adds too many details of ship construction and cost, as well as business data that could have been buried in a footnote. American business historians would not suffer greatly, for instance, if they did not know that "Thornton Paillon (or Paillou)" was one of Charles Mallory's earliest journeymen (p. 18). Trivial details of this sort become especially bothersome as Baughman traces the Mallorys into the twentieth century, when their undertakings became more complex and at the same time less closely attuned to the main lines of American economic development.

These are, however, minor quibbles about an otherwise superb volume, clearly one of the best business histories of the past decade. Unlike many students of firm history, Baughman reports the bad news along with the good. He makes it quite clear that the Mallorys spent most of their career in a market protected by their government from foreign competition; cabotage was, in this case, the keystone of capitalism. In the early years of the nineteenth century, as Baughman shows, the family built a fortune that was in part wrung from labor through an exploitive system for paying wages. The author tells us about the Mallorys' mis-

takes as well as their successes, matching the debits suffered when the Brazil steamship line collapsed against the assets piled up in the trade between Galveston, Texas, and New York.

Most important are the clarity, authority, and honesty that the author displays when he is placing his subjects in their historical context. Baughman never exaggerates the extent to which the Mallorys were innovators. As he pictures them, they were rather cautious men who followed when the path was clear. This was true even in the first generation when Charles Mallory was founding the family fortune. This, I believe, will be the section of the book that will be of greatest interest to economic and business historians and, indeed, to all those concerned with America in the first half of the nineteenth century. Here Baughman does a truly outstanding job of showing the complex inner workings of an economy in transition. He blends history with micro- and macro-economic analysis, while never letting the reader forget that the subject at hand is a particular New England family that made its wealth at sea.

LOUIS GALAMBOS  
*Johns Hopkins University*

JAMES D. HORAN. *The McKenney-Hall Portrait Gallery of American Indians*. New York: Crown Publishers. 1972. Pp. 373. \$29.95.

THOMAS L. MCKENNEY. *Sketches of a Tour to the Lakes, of the Character and Customs of the Chippewa Indians, and of Incidents Connected with the Treaty of Fond du Lac*. Reprint: Barre, Mass.: Imprint Society. 1972. Pp. xx. 414. \$40.00.

Thomas Loraine McKenney was a key figure in American Indian-white relations. As superintendent of Indian trade from 1816 to 1822 and then as head of the War Department's Indian bureau from 1824 to 1830, he had a dominant influence on trade relations with the Indians, on treaty negotiations, on education and civilization programs, and on Indian removal. Moreover he began a national museum of Indian artifacts and collected a remarkable gallery of Indian portraits that later formed the basis for one of the first great lithographic publications in the United States. With such a career it is hard to understand why he has remained so

shadowy a figure. Now, it appears, he is emerging from his obscurity.

James D. Horan's volume reproduces in color 125 portraits of Indians from the *History of the Indian Tribes of North America*, which McKenney published with James Hall between 1836 and 1844. Included also is a lengthy biography of McKenney. The book is grandly conceived and to the casual reader will supply a useful introduction to McKenney and his significant work. Yet the volume has many faults and will disappoint scholars of Indian affairs and of American art. The color reproductions, some of which are poorly done, were not all taken from the magnificent lithographs of the first edition of the *History*; many come from a later edition that did not capture the splendor of the original work. Horan has omitted the biographical and ethnological sketches of the Indians that McKenney and Hall supplied for their work and replaced them with brief biographies of his own. He has thus, unfortunately, eliminated much of the historical value of the original.

Horan's biography of McKenney is based on a wide array of primary sources and furnishes valuable insight into the man's character and career. It also tells a good deal about early American Indian policy. But the style is anecdotal—interesting stories get much more emphasis than they warrant—and sustained development is frequently lacking. Factual and typographical errors in the text, the notes, and the bibliography give evidence of carelessness or haste; they require that the book be used with caution.

McKenney's own *Sketches of a Tour of the Lakes*, now reprinted in splendid and expensive form, is the account of his excursion to the head of Lake Superior with Governor Lewis Cass of Michigan Territory in 1826 to conclude a treaty with the Chippewa Indians. It is a chatty and effusive book, which is one of the best accounts available of early treaty negotiations. It also reflects McKenney's concern for Indian affairs and his interest in Indian customs. An admirable introduction to this edition by Herman J. Viola sets the account in its historical perspective and tells the publication history of the book.

FRANCIS PAUL PRUCHA  
*Marquette University*

SYLVIA E. CRANE. *White Silence: Greenough, Powers, and Crawford, American Sculptors in Nineteenth-Century Italy*. Coral Gables, Fla.: University of Miami Press. 1972. Pp. xviii, 499. \$9.95.

Three expatriate American sculptors, torn in their loyalties between the States and Italy "like the ass between 2 bundles of hay," as Horatio Greenough described his condition, are the subject of Sylvia Crane's study. Based almost entirely on manuscript sources, Crane's triple biography adds many details to our knowledge of the day-to-day activities of Greenough, Hiram Powers, and Thomas Crawford. In this sense it supplements the work of Nathalia Wright on Greenough and Robert Gale on Crawford and makes available as yet unpublished correspondence and writings of Powers. But this additional information does not contribute much that is new to our understanding of the work of these men or to the expatriate experience. Because the author in her single-minded concern with manuscripts has ignored recent studies in the history of American esthetic theory and the social history of American art, she fails to place these men within longstanding American esthetic traditions or achieve deep understanding of the cultural complex in which they participated. In particular, she falls into the error of accepting as true the myths, fostered to a great extent by the artists themselves, concerning American indifference to the arts and the nation's "materialism" at this time in its history.

Greenough, Powers, and Crawford went to Italy to study and practice the art of sculpture because Italy afforded excellent and inexpensive marble, cheap skilled workers, and presumably an encouraging atmosphere for art. There they continued to work in the classical mode that dominated late eighteenth- and early nineteenth-century taste. Crane asserts that the three sculptors were responsible for "the transmission back home of the Italianate neoclassical aesthetics of their epoch" (p. xiii)—a questionable claim since the irony of their careers is that esthetic interest in the United States was moving away from neoclassicism even while the sculptors were achieving eminence in its practice. Greenough and Crawford returned home for a sufficient length of time to catch a whiff of the new romanticism before returning to the

sterilities of the older mode, and their later works and writing reflect their efforts to understand creatively some of its concepts. Greenough's functionalism, then, was derived as much from contemporary nature thought and his seventeenth-century Puritan past as these were embodied in Transcendentalism as from the theories of the German neoclassical critic Johann Winckelmann or the Italian architect Francesco Milizia. Crawford's belated efforts to define an American esthetic grew out of his attempt to fulfill governmental commissions that would reflect the American spirit as he understood it. Powers's art, on the other hand, did not develop beyond the imitative because he never returned to the land that had encouraged his best artistic skills. These sculptors did not so much carry the neoclassical ideal to America, where it had already made its converts, as they were encouraged by the fresh winds blowing in America to develop their most interesting and significant artistic insights.

Useful for its primary materials, Crane's book combines some new facts with outmoded interpretations and old errors. Since much of it involves repetition of what has been said well by others, and since there is so much still in American art history that requires exploration, it is regrettable that scholarly energy and intelligence should have been expended on a subject already adequately treated.

LILLIAN B. MILLER  
*National Portrait Gallery*

JOHN W. BLASSINGAME. *The Slave Community: Plantation Life in the Antebellum South*. New York: Oxford University Press. 1972. Pp. xv, 262. \$7.95.

This book promises new insights concerning the nature of slave life utilizing the trifocal perspective of slave, planter, and traveler. Unfortunately these aims are imperfectly realized, primarily because Professor Blassingame lacks a clear analytical perspective. The absence of a conceptual focus is expressed at all levels of the book from the ordering of chapters, through the muddled, often repetitious, discussion of issues within chapters, to confusion about the use of borrowed social science concepts. Most important, chapters 5 through 7, which dis-

cuss the nature of the plantation system and its consequences for slave activities, relationships, and personalities, should precede a discussion of the topics of chapters 1 through 4, which discuss leisure activities. African survivals in slave culture, resistance to slavery, and slave family life. Without a systematic institutional framework, the significance for slave life of the topics discussed in the first four chapters cannot be assessed. Nevertheless, Blassingame's discussion of three slave personality stereotypes in antebellum Southern literature is fascinating; his concluding historiographical essay provides a useful evaluation of primary and secondary sources on slavery; his methodological aims are important if not systematically pursued: some of his assertions are better considered as hypotheses for further study (for example, the prevalence of Sambo personalities among house slaves and on small plantations). Blassingame's overall contribution to the understanding of slave life, therefore, is frustratingly uneven.

A serious substantive weakness in Blassingame's description of slavery is his underplaying of the coercive role of blacks while he deals exhaustively with their resistance to slavery. In describing the enslavement of blacks in Africa, Blassingame mentions only indirectly the role of Africans in this process, though an appreciation of that role is essential for understanding the slave trade. Similarly, while Blassingame refers to the driver as part of the authority system of the plantation and deals extensively with the coercive role of the planter and the overseer, the author does not cope in any way with the social and psychological complexity of the black driver's role. Blassingame's failure to analyze the coercive role of blacks necessarily distorts his portrayal of slave life.

The most intriguing promise of Blassingame's book is the utilization of a trifocal perspective on plantation slavery. Although this perspective is not systematically developed, the viewpoints of slaves and planters are clearly articulated, while that of the traveler *qua* traveler never receives distinctive treatment. In the preface Blassingame suggests that scholars have rarely used slave autobiographies to understand slavery; while this may be true in general, it would be disingenuous not to recognize E. Franklin Frazier's seminal paper of

1930, "The Negro Slave Family," which is based on slave autobiographies, and such collections of slave autobiographies as Gilbert Osofsky's *Puttin' On Ole Massa* (1969) or George P. Rawick's recently published *From Sundown to Sunup: the Making of the Black Community*. As its title suggests, Rawick's insightful and analytically sophisticated study shares many of the aims of Blassingame's book; moreover, it is based upon the narratives of former slaves, narratives collected under the auspices of the WPA from people who were more representative of most slaves than the exceptional slave autobiographers on whom Blassingame relies.

Blassingame's *The Slave Community* promises more than it achieves, in part because his intellectual integration of social and psychological orientations has yet to be fully achieved, and in part because he attempts to satisfy a variety of audiences from scholarly peers to students in introductory black studies courses.

MARION D. DE B. KILSON  
Newton College

BETTY FLADELAND. *Men and Brothers: Anglo-American Antislavery Cooperation*. Urbana: University of Illinois Press. 1972. Pp. xiv, 478. \$11.50.

PENELOPE CAMPBELL. *Maryland in Africa: The Maryland State Colonization Society, 1831-1857*. Urbana: University of Illinois Press. 1971. Pp. vi, 264. \$7.95.

Frederick Jackson Turner's shade still lingers in the American historical consciousness. Like the great historian himself, most scholars search for domestic, purely American roots to explain national character and institutions. On the whole we tend to forget our commonality with transatlantic peoples, particularly the British, an interconnection that persisted long after formal ties were broken. Betty Fladeland's *Men and Brothers* challenges the parochial view, adding a political dimension to David B. Davis's brilliant studies of an evolving antislavery concept in Western culture. A model of thorough research, *Men and Brothers* reminds us that America, especially in its religious and reform tradition, participated in the vast social and intellectual transformation of the Atlantic littoral from the days of the Stuarts to the reign of Victoria and Albert.

Fladeland early sets the sympathetic tone of the work: "I count the antislavery advocates as worthy of respect" for their bold efforts in behalf of recognition of human worth. Although Fladeland admits that abolitionists could be "petty" and "overbearing" (p. xiii) whether they ate mutton or Boston beans, less reverence and more candor in the analysis would have sharpened the critical edge. Yet the author has fully mastered the inherent difficulties of conceptualization. Especially skillful is her account of the interplay of reform and self-interest, domestic politics and foreign relations, in the almost simultaneous outlawing of the international slave trade in both countries. Nor is she wholly uncritical. American abolitionists, Fladeland observes, occasionally fell into self-satisfied lassitude, most crucially at the time of the passage of a proslavery Constitution.

On the whole, Fladeland concludes, Anglo-American reform stimulated activity and exchanges of methods, but a price was exacted. As sectional feelings hardened, Southerners grimly warned of British imperial designs disguised in pious antislavery rhetoric. West Indian emancipation in 1833 intensified American nativism, proslavery fears, and mob hostilities. At the same time, the British measure radicalized and heightened the frustrations of American reformers, impressed by the success of "immediatist" demands abroad in contrast to unchecked slave expansion here. Throughout Fladeland reveals how British interest in American slavery, especially after 1833, became a significant factor in the rise of sectional antipathies leading to war.

To her credit, the author traces the parallels and continuities of Anglo-American reform, so often artificially divided by nation, chronology, and sectarian strategy. Yet the texture of an emerging transoceanic Victorian culture with its mass communications and uniformities is rather faintly portrayed. Moreover the theme of continuity hides the shifting class basis of transatlantic reform leadership. English antislavery became an increasingly strident social protest of a dissenting middle class against Tory hegemony, one reform among various measures to diminish feudal privilege. Parallel with the English trend, the American "wise and good" of enlightened benevolence relinquished antislavery to a new breed of evangeli-

cal romanticists in the late 1820s. Nevertheless, Fladeland has corrected a longstanding error: the view that American reform was cultivated in native soil without foreign nourishment.

Penelope Campbell's *Maryland in Africa* could well serve as an appendix to Fladeland's impressive study. Maryland's colony was, in a sense, a microcosm of transatlantic reform interests derived, as it was, from Britain's experiment at Sierra Leone. Campbell lacks Fladeland's style, complexity, and insights, but she shows that colonizationists were considerably less inhumane than Garrisonians so loftily proclaimed. One must admire the persistence of John H. B. Latrobe and James Hall as they strove to make the settlement a success. Fear of border-state free Negroes prompted this offshoot of the American Colonization Society, but Maryland philanthropists honestly endeavored to give freedmen an opportunity for selfhood unavailable in racist America. While blessed with fertile land and ordinarily peaceable African neighbors, the Cape Palmas settlers developed neither a political nor an economic resiliency. Finally they were absorbed into Liberia in 1857. Apparently the Negro colonists shared their former masters' aversion to manual labor, taste for alcohol and revivals, and contempt for native Africans, sometime victims of American black oppression. If this outpost was a tiny replica of Fladeland's world of Anglo-American culture, Campbell's interesting study suggests that white and black Americans, as well as Englishmen, evinced similar tendencies for imperial ambition, humanitarian paternalism and religiosity, along with some gentler traits that Turner had so eloquently celebrated.

BERTRAM WYATT-BROWN

Case Western Reserve University

DOROTHY M. JOHNSON. *The Bloody Bozeman: The Perilous Trail to Montana's Gold*. (The American Trails Series.) New York: McGraw-Hill Book Company, 1971. Pp. xx, 366. \$9.95.

*The Bloody Bozeman* is a chronicle of American occupation of the Northern Plains and Rockies, centering on Montana Territory, during the period 1862-68. The mining frontier entered this region in 1861 when a party of Californians found gold on a tributary of the



Clearwater in Idaho. By 1862 prospectors were ranging eastward into Montana. In July, John White found gold on Grasshopper Creek, miners swarmed to the Montana diggings, new fields were discovered including the fabulous Alder Gulch, and before the close of 1862 miners had established a cluster of camps across southwestern Montana centering on Virginia City. Several roads connected this remote gold mining region with settlements in Washington Territory, Salt Lake and Denver, and the East. The principal artery for access to the Montana gold camps was the Oregon Trail. Laterals from this transcontinental emigrant highway, including Bridger's Road, ran north to Virginia City and the peripheral camps. These roads were relatively free of Indian attack but, because rough terrain deflected direct routes to the camps, they followed time-consuming oblique courses. In 1863 John Bozeman blazed a more direct passage from the Emigrant Road at Deer Creek Crossing on the North Platte River to the Montana mines, reducing the passage by 400 miles. For the benefit in time and travel saved, Bozeman Trail travelers paid the price of great risk, for it coursed through the domain of the fierce Sioux. In 1866 the United States Army fortified the road with three military stations manned by cavalry and infantry. Red Cloud and his Sioux warriors bitterly contested this passage and regularly confronted army units guarding the road. In military annals these include the Wagon Box Fight and the Hayfield Fight. Relentless Sioux pressure forced abandonment of the military posts and the Bozeman Trail in 1868 as the principal route to the Montana mines.

*The Bloody Bozeman* is a melange of personal experiences, military and civil, along the Bozeman Trail and in the mining communities the road served. Through this medium the author presents engaging glimpses of life in the mining camps, mining camp law, vigilantes, and the Henry Plummer episode. The book's seemingly disparate, episodic, sketch-type content is laced into a provocative, if at times elusive, continuity by the Bozeman Trail theme. It is written in a familiar, homey style, drawn from letters, diaries, and other private primary sources of the miner and military pioneers along the Bozeman Trail. The work closes

with an epilogue section titled "What Happened to Some of Them," the principals of the drama along the Bozeman Trail, including John White, who made the initial strike at Grasshopper Creek, John Owen, Franklin Kirkaldie, the ubiquitous Jim Bridger, and Thomas Francis Meagher, international Irish revolutionary figure and acting governor of Montana Territory.

ARRELL M. GIBSON  
*University of Oklahoma*

PHYLLIS DAIN. *The New York Public Library: A History of Its Founding and Early Years*. [New York:] New York Public Library, Astor, Lenox and Tilden Foundations. 1972. Pp. xix, 466. \$15.00.

C. H. CRAMER. *Open Shelves and Open Minds: A History of the Cleveland Public Library*. Cleveland: Press of Case Western Reserve University. 1972. Pp. x, 279. \$9.95.

For many years American library history was neglected by historians and librarians alike. Most of the books published were laudatory biographies or fact-filled institutional histories, frequently written from a provincial point of view and displaying more sentiment than scholarship. In the past decade, however, the situation has improved dramatically. Serious researchers are producing, with increasing regularity, studies that view library development as an integral part of American cultural history. The histories reviewed here take this wider view. Each author has made an effort to examine the political and social forces that shaped the institution. As a result, their volumes are useful syntheses that avoid many of the defects of traditional library history. Phyllis Dain's book is of particular significance.

Dain is a professor in the School of Library Science of Columbia University, where this work was first written as a doctoral dissertation. The first volume in a comprehensive history of the New York Public Library, it is described by the author as "an analysis of the decisive first years . . . viewed against the history of New York City and its library conditions." It also "complements and to some degree supersedes" Harry M. Lydenberg's history of the same institution (*History of the New York Public Library* [1923]), a task skillfully accomplished, for Dain not only updates Lydenberg,

she supplements the earlier volume by emphasizing subjects that he treated in a sketchy manner.

Professor Dain concentrates on the merger of the privately financed Astor and Lenox Libraries with the Tilden Trust, which established the New York Public Library in 1895, and the initial organizational efforts of its formidable first librarian, John Shaw Billings. In 1901 NYPL began absorbing newly created, tax-supported circulating libraries. The resulting growth of the circulation department, the planning and construction of the central building at Fifth Avenue and Forty-second Street, and Andrew Carnegie's gift of branch library buildings are also detailed. Even when discussing personalities or local politics, she stays focused on the processes that ultimately determined the library's unique character; the result is a well-organized, readable narrative. Despite her efforts to link the library and the history of the city in a direct manner, organizational details dominate; consequently, the volume must be viewed primarily as administrative history. But it is an admirable analysis, copiously documented with eighty-four pages of scrupulous notes. This handsome volume will endure as the authoritative account of the early years of a remarkable institution.

While informative, C. H. Cramer's centennial history of the Cleveland Public Library (1869-1969) is less successful. His book is especially disappointing since it is the first full history of the library and because Cramer—a professor at Case Western Reserve University—is an experienced historian. The problem is his limited purpose: "to attract interest in the major events, whether they are triumphs or failures, in the record of the Cleveland Public Library." This "highlights" approach may be suitable for the general reader, but it is too superficial to satisfy most historians. The volume was commissioned by the Friends of the Cleveland Public Library.

Despite his limited approach, Cramer has produced an interesting, crisply written volume. "Open shelves" for public perusal was an innovation of William Howard Brett, the distinguished librarian from 1884 to 1918. Cramer provides fine character sketches of Brett and John Griswold White, a fascinating book collector and library trustee who also exerted

great influence during the institution's early years. The author's frank description of the disastrous impact that narrow-minded city politicians had on the library during the 1930s is intriguing and thought-provoking. Unfortunately his coverage of later years is both inadequate and uneven, lacking the more balanced approach of earlier chapters. Skillful use is made of official documents and local newspaper accounts, and the forty-two pages of notes contain many colorful anecdotes, often based on personal interviews. Cramer's history is a useful and generally well-integrated overview, but it does not go far enough. A thorough analysis of the Cleveland Public Library and its importance to Cleveland and the American public library movement is still needed.

JOHN Y. COLE

*Library of Congress*

MARVIN LAZERSON. *Origins of the Urban School: Public Education in Massachusetts, 1870-1915*. (Publication of the Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xix, 278. \$10.00.

ARTHUR G. WIRTH. *Education in the Technological Society: The Vocational-Liberal Studies Controversy in the Early Twentieth Century*. (The Intext Series in Foundations of Education.) Scranton, Pa.: Intext Educational Publishers. 1972. Pp. xi, 259. Cloth \$8.75, paper \$5.25.

EDWARD A. KRUG. *The Shaping of the American High School. Volume 2, 1920-1941*. Madison: University of Wisconsin Press. 1972. Pp. xv, 375. \$15.00.

In the urban Massachusetts schools of 1900, classes of sixty children were common, sanitary conditions were often primitive, and forty per cent of the students took longer than six years to pass through the first six grades. Yet educators worked in a climate of buoyant faith about the possibilities of the public school system. Schools would wean immigrant children away from the bad influences of street life and their own parents, offering them hope for social mobility. Education was linked to goals of productivity and efficiency which seemed entirely appropriate to an industrial society. In American schools today class sizes are smaller, teachers are better trained, physical plants

usually somewhat more adequate. The faith, however, has largely evaporated. Cynicism and defeatism are far more prevalent, and fundamental questions are raised about the value of the undertaking that would have been unthinkable in earlier discussions.

These three books take us back to the age of confidence in education. Marvin Lazerson gives us a solid look at ten large Massachusetts school systems in the years from 1870 to 1915, as seen primarily through the eyes of those who ran them. His book is more descriptive, less thesis-ridden than Michael Katz's *The Irony of Early School Reform* (1968), which covers the mid-nineteenth century. Lazerson's book is grounded in a hard sense of social realities. Rhetoric is measured against concrete results. Like Katz, Lazerson emerges with a negative verdict: "Challenged to be relevant, educators adopted programs that claimed more than they could accomplish, that were soon found wanting, and that, once institutionalized, affected the schools far more than society's ills" (pp. 249-50). The broad moralism of the late nineteenth century was replaced by vocationalism and patriotic citizenship training. Vocationalism was supposed to bring the schools closer to the practical world of industry, but it failed to make a real connection; instead, its main effect was increasingly to segregate the classrooms of the poor from those of the middle class. Educators with limited budgets were too greatly concerned about making some impact in the slums to face the issue of declining democracy. Lazerson has produced an impressive, well-written monograph.

Arthur G. Wirth reviews the national debate over vocational education in the Progressive era. Steeped in the older Deweyan educationist tradition, he bravely tries to come to terms with newer perspectives, welcoming Paul Goodman as a Dewey for our times. He sees the battle as one between narrow, business-oriented advocates of social efficiency and humanistic utilitarians like Dewey. Despite occasional breadth of vision, the book plods. On page 173 Henry James (not Henry Adams) is credited with positing the contrast between the virgin and the dynamo.

By 1920 one-third of American youth were attending high school, and school leaders were pushing hard to make attendance universal,

armed with the slogan of social efficiency. Edward A. Krug, author of a highly regarded history of the high school before 1920, now continues the account down to Pearl Harbor. His research is wide-ranging and resourceful. In lucid prose he presents abundant material for a devastatingly negative appraisal of American secondary education in this period, and of the lackluster schoolmen who carried on third-rate intellectual debates (mainly quarreling over specific varieties of anti-intellectualism) while simultaneously bolstering their empires. But, except regarding the social conformism of the 1920s, Krug shrinks from quite making the indictment. A level-headed tone of description predominates. He has really written two quite different books, one on each decade. The treatment of the 1920s is far superior. We learn something of what students and teachers were actually like, and of concrete curricular practices as well as abstract philosophies. In discussing the 1930s Krug loses this focus, largely confining himself to an endless rehash of verbal controversies within the ranks of educators. We get a much clearer picture of the various factions than we do in Lawrence Cremin's *The Transformation of the School* but wonder if their rhetoric is worth so much attention. Had Krug stuck to social history, his book would be as consistently engaging as it is authoritative. It is noteworthy that in it Freud's name is not mentioned once.

Regardless of their authors' intentions, all three books will heighten the impression that American schools of the early twentieth century were generally mediocre and that their leadership dwelt in a curious cloudland.

LAURENCE VEYSEY  
University of California,  
Santa Cruz

DUNCAN LYLE KINNEAR. *The First 100 Years: A History of Virginia Polytechnic Institute and State University*. Blacksburg: Virginia Polytechnic Institute Educational Foundation. 1972. Pp. xiv, 498. \$6.95.

S. ARTHUR WATSON. *Penn College: A Product and a Producer*. Oskaloosa, Iowa: William Penn College. 1971. Pp. xiv, 417. \$7.50.

WILLIAM LLOYD FOX. *Montgomery College: Maryland's First Community College, 1946-1970*. [Rockville, Md.:] Montgomery College. 1970. Pp. xi, 115. \$1.95.

J. MARTIN KLOTSCH. *The University of Wisconsin—Milwaukee: An Urban University*. [Milwaukee:] University of Wisconsin—Milwaukee. 1972. Pp. xiii, 151. \$5.00.

The histories under consideration are concerned with four types of American colleges and universities that have been conspicuous in the rapidly changing educational scene since World War II. Two are centenarians that have been substantially altered in recent years, and two are emerging or developing institutions.

Each was intended to fulfill a designated "mission." Virginia Polytechnic Institute, founded in 1872, was the Virginia land-grant college, and it was deliberately separated from the older state university to ensure that education for farmers and mechanics would not be subverted by the older, elitist-professional education offered in Charlottesville. William Penn College, opened a year later in Oskaloosa, Iowa, was to provide sectarian-oriented training as well as learning for the children of a Quaker community in a limited geographical area. Montgomery Junior College was launched in 1946 to provide a terminal vocational education as well as the conventional preuniversity junior college curriculum for students of a suburban community of Washington, D. C., and to bring that education practically to their doors. The University of Wisconsin—Milwaukee, organized in 1955, is illustrative of a related movement whereby the state universities would be brought within easy reach of large population centers.

Like so many others of its kind, Wisconsin—Milwaukee began as a tightly controlled and subordinate branch of a jealous parent university. From the very beginning the leaders of the new institution have striven for independence on the ground that only by separation could they discharge the "urban mission" called for by its situation. Yet that mission has proved harder to define and to implement than it has been to announce. In fact, the Milwaukee university is already showing increasing resemblance to Wisconsin—Madison, including a foreign assistance program in which the chancellor-author takes great pride. They are even building dormitories at Milwaukee in order to vary the student mix by attracting nonresident students!

American institutions of higher education

tend to become increasingly alike. VPI exemplifies the once separate agricultural and mechanical colleges that have evolved into state universities not greatly different from the original establishments. Actually the process has been that of both types moving toward each other. It is probable that most of the community colleges can resist the powerful urge to upward mobility and homogenization. Yet the terms "junior" and "community" have been successively dropped from Montgomery's official name, and its historian may be giving us a glimpse of things to come when he makes a point of informing us that the college is now the second largest higher education institution in Maryland and that it appears headed for an expanding future.

If William Penn's example means anything, there is certainly little future in standing still. Unable to obtain any significant amount of public support, even from its own locality, and still dependent upon a regional denominational community that is in decline, Penn exemplifies the dilemma of a great many sectarian colleges unable to break loose from their original mission after it has lost broad appeal. Although it is presently in official good standing with the regional accrediting association, Penn has lost accreditation twice in its history, and it faces the grim realization that a great many institutions like it have gone under.

Only one of these books has much merit as a full-fledged history. Professor Kinnear of VPI knows a lot about his institution, and he loves it; he has done extensive research, writes with considerable vitality, and has provided a bibliography and index. A good editor might have given help on organization, at least to the extent of varying chapter titles, which plod on and on, administration by administration.

The history of Wisconsin—Milwaukee, nicely packaged, with all of the scholarly apparatus of citations, bibliographical note, and index, has some of the self-justifying tone of a corporation report. Written and published by the chancellor who has headed the institution since it was organized seventeen years ago, the book does have some worth as a memoir of the man who knows more firsthand about his subject than anyone else.

Montgomery College wasted the talents of a professional historian. It does not appear that

Professor Fox had full access to all of the material necessary for a full study, although, having been on hand since the college opened, he has managed to produce a readable and informative work within the limitations imposed. The book itself is cheaply done, and there is neither index nor bibliography.

The general level of the history of William Penn is apparent in the subtitle: *Product and Producer*. It is confusingly organized and loaded with lists of students, donors, and what-not. The most useful sections are those on Quaker settlement in southeastern Iowa and on the Quaker academies in the region; only one survives of the eighteen that were founded.

LOUIS G. GEIGER  
Iowa State University

WILLIAM S. POWELL. *The First State University: A Pictorial History of the University of North Carolina*. Chapel Hill: University of North Carolina Press, 1972. Pp. x, 309. \$12.95.

In an impressive volume whose format, dust jacket and all, can only be described as opulent, William S. Powell has brought together an interesting and remarkable collection of pictures in *The First State University: A Pictorial History of the University of North Carolina*. The author refers to the volume as "a picture book." It is certainly that, and an excellent one, rather than a history. That critical material is lacking is to be expected, but there is no index, a real necessity in a volume containing several hundred illustrations. There is a full page of credits indicating the source of much of the pictorial material. Looking over its three hundred pages, the reader feels that here is a book of plates designed to supplement the text of a more traditional history of Chapel Hill. The organization of the book suggests that it is designed for viewing rather than reading.

Starting with a few pages of uninterrupted narrative dealing with the launching of the university, the reader has then to jump back and forth across the two-column pages to read the captions accompanying the illustrations. Between some of these captions there is a degree of continuity; with others, particularly the many useful biographical notes, there is none. Reading is made difficult, and because of the

lack of an index, use for reference is made even more difficult.

No criticism of the form of the book can lessen respect for the author's industry, good taste, and skillful collection of such a vast amount of graphic material, so excellently reproduced. Future historians of the University of North Carolina and countless unnamed writers of promotional material will pilfer shamelessly from Powell's vast store of material illustrating the activities of the university and its people from the beginning of its history to the present day.

ELMER LOUIS KAYSER  
George Washington University

NORMAN CLEAVELAND, with GEORGE FITZPATRICK. *The Morleys—Young Upstarts on the Southwest Frontier*. Albuquerque: Calvin Horn Publisher, 1971. Pp. xi, 269. \$7.50.

A glance at the literature about the Southwest in the 1870s would not seem to indicate that William R. Morley was a figure of historical importance, but he was a man of some significance and even greater promise until his untimely death at the age of thirty-six. Like so many other veterans, Morley went west after the Civil War and worked as an engineer for the Kansas Pacific Railroad. He became manager and later vice-president of the Maxwell Land Grant Company in New Mexico Territory, a position that did not prevent him from supporting the interests of the settlers or from becoming one of the earliest and most vocal critics of the Santa Fe Ring. He was also one of the editors of the *Cimarron News and Press*. Later, as an employee of the Atchison, Topeka and Santa Fe Railroad, Morley was a key figure in the race against the Denver and Rio Grande for control of Raton Pass and Royal Gorge and also selected the Santa Fe's route through Sonora.

Although centered around William Morley, this book is a family history by William Morley's grandson and is based on family reminiscences supplemented by research in primary and secondary materials. Although it is occasionally rambling and disorganized and should be used in conjunction with other books, such as Robert Athearn's *Rebel of the Rockies*, Howard Lamar's *The Far Southwest*, and Jim Pearson's



*The Maxwell Land Grant*, it does provide some new perspectives on the history of the Southwest and may be of use to regional historians.

RICHARD N. ELLIS  
University of New Mexico

KENNETH E. DAVISON. *The Presidency of Rutherford B. Hayes*. (Contributions in American Studies, number 3.) Westport, Conn.: Greenwood Press, 1972. Pp. xvii, 266. \$12.00.

Perhaps no historian has studied the life of Rutherford B. Hayes with more care and devotion than Kenneth Davison, professor of history at Heidelberg College. In a useful article in *Ohio History* ("The Nomination of Rutherford Hayes for the Presidency," 77 [1968]: 96-110) and in several convention appearances, Davison has impressed scholars with his meticulous knowledge of the nineteenth president and his family, the result largely of a decade of labor in the rich collections at the Hayes Library in Fremont, Ohio.

The major contribution of this volume is not its analysis of the issues confronted by the Hayes administration but its description of those who participated in the administration. Lavish attention is paid to the Hayes family, whom Davison admires without reservation. (At times this approach results in treacle. Of Lucy Hayes, for example: "She sang beautifully and even her call to breakfast—'Familee-ee-ee'—was sweet and full voiced.") There is no better single source of biographical information in print on the members of Hayes's cabinet. The White House staff—right down to domestic servants—is described at length.

Davison has collected a great many details about the operating costs and procedures at the White House that will attract some readers. One can learn, for example, the costs of banquets and carriage horses, the salaries of cooks, or how many laundresses were employed at one time. He surely has corrected the view, popular during the 1880s, that the Hayeses had been extremely reluctant to entertain.

On the larger matters chapters are almost exclusively terse summaries of the scholarship of others. The most positive interpretations prevail. Accounts of the campaign of 1876, the ad-

ministration's approach toward the South, and Carl Schurz's Indian policy are solid and make interesting reading. The author's understanding of the New York customhouse controversy, on the other hand, leaves much to be desired. His thin treatment of the silver issue and the railroad strikes of 1877 will disappoint many historians of the period. The contention that Hayes left the nation united, prosperous, and happy is, to say the least, debatable.

The volume contains an extensive bibliography, and includes a valuable list of dissertations written on relevant topics. A note on sources briefly describes the major collections in the Hayes Library.

THOMAS C. REEVES  
University of Wisconsin—  
Parkside

KEITH L. BRYANT JR. *Arthur E. Stilwell: Promoter with a Hunch*. Nashville: Vanderbilt University Press, 1971. Pp. xi, 256. \$10.00.

Other than his autobiographical forays in the 1920s, the only published knowledge of Arthur Edward Stilwell was presented by David M. Pletcher in his *Rails, Mines, and Progress: Seven American Promoters in Mexico, 1867-1911* (1958). *Arthur E. Stilwell* is, therefore, a welcome addition to the knowledge of this promoter's life as well as a welcome addition to the growing published literature dealing with the less spectacular businessmen of the nineteenth and early twentieth centuries. Knowledge of Stilwell and others like him is increasingly illuminating the nature of American economic growth and power, and the inevitable result will be a marked reconsideration of views of businessmen in this period. Arthur E. Stilwell certainly does not comfortably fit the interpretations of many American historians.

Stilwell entered the business community and successfully pursued careers in insurance, real estate investment, railroading, and writing. Most notably, after 1890 he conceived and guided railroad projects that resulted in two major lines with 2,300 miles of track—this when the American railroad network had been substantially completed and railroads were losing the confidence of the investing public. The Kansas City, Pittsburg, and Gulf Railroad connected Kansas City to Port Arthur, an in-

land port Stilwell created on the Gulf of Mexico. When he lost the KCPG to Edward Harri-man and John Gates about 1900, Stilwell began the Kansas City, Mexico, and Orient Railway, which was to connect Kansas City to Baja California. Stilwell lost control of this enterprise long before it was completed by the Mexican government in the 1960s.

After his unwilling retirement from business Stilwell turned to writing on a wide variety of subjects. His two most successful books were *Cannibals of Finance* (1912), which castigated the machinations of "Wall Street," and *Confidence or National Suicide* (1910), which defended the railroads against increased federal control.

One of the particularly interesting aspects of this well-researched work is chapter 2—"The Promoter and His Methods." Here the author relates Stilwell to some key theories and definitions of economic history, e.g., was Stilwell an entrepreneur or a manager? In comparison to similar biographies the reader therefore finishes the book with a surer grasp of what Stilwell was and what he was not.

BRIT ALLAN STOREY  
State Historical Society  
of Colorado

B. JOYCE ROSS. *J. E. Spingarn and the Rise of the NAACP, 1911-1939*. (Studies in American Negro Life.) New York: Atheneum. 1972. Pp. xii, 305. \$10.00.

This is more than a fine study of Joel Spingarn's role in the NAACP: it is the best account we have of the association during its first three decades. Spingarn joined the fledgling NAACP in 1910 and was elected chairman of the board of directors in 1914 to help heal an internal schism. One of the few white NAACP officers not of abolitionist descent, Spingarn nevertheless coined the phrase "New Abolition" to express the association's philosophy of uncompromising opposition to segregation. Ironically, it was Spingarn who in 1917 almost singlehandedly persuaded the War Department to create separate training camps for black army officers (as the only alternative to no officer training at all for Negroes), an achievement that caused embarrassment in 1934 when he opposed Du Bois's proposals for black self-

segregation as an instrument of social and economic progress. After distinguished combat service in World War I, Colonel Spingarn returned home broken in health and disillusioned by postwar reaction. His participation in the NAACP declined for a few years, but he did serve as treasurer in the 1920s and was elected in 1931 to the dual positions of president and chairman of the board, from which he steered the association through the difficult depression years until a brain tumor curtailed his activities and brought death in 1939.

Professor Ross has made good use of sociological studies of leadership and organizations to inform her interpretation of Spingarn and the NAACP. She is especially skillful in portraying Spingarn as an "interpersonal leader" who mediated factional and personal feuds within the NAACP. Ross also analyzes effectively the NAACP's evolution from an *organization* to an *institution* in the 1920s, a process that centralized control, ossified strategy, and inhibited the association's capacity to respond flexibly to black economic needs during the depression. Less successful is the author's attempt to apply the concept of "noneconomic liberalism" to the NAACP. True, many of the association's white leaders fit Seymour Lipset's definition of noneconomic liberals whose education, status, and psychic security enabled them to focus on issues of civil liberty rather than social welfare. But some of them were also socialists or otherwise active in economic and social reform movements. And to argue that noneconomic liberalism was the primary reason for the NAACP's continuing emphasis on civil and political equality slights the importance of other factors (especially the desire to avoid duplication of the National Urban League's efforts), fails to explain why socialists such as Mary White Ovington insisted that the NAACP concentrate on civil rights, and begs the question whether black leaders who took the same position were indeed noneconomic liberals. In any case, Ross's discussion of the crisis that led to Du Bois's resignation in 1934, of the consequent debate over future strategy, and of the gradual transition from white to black leadership during Spingarn's three decades of service to the NAACP are the best treatments of these matters in print. It is no easy task to write the history of a biracial or-

ganization; Ross has performed the task with grace and clarity.

JAMES M. MCPHERSON  
Princeton University

SIDNEY BELL. *Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. 209. \$10.95.

Professor Bell, a Wisconsin Ph.D., presents still another New Left account of American diplomatic history. He notes that much of the debate about Wilson's role at the Paris Peace Conference has focused upon the manner in which Wilsonian goals were modified by Allied objectives rather than upon the nature of those goals themselves. He fails to refer to the recent careful study by N. Gordon Levin, Jr., *Woodrow Wilson and World Politics* (1968), which while emphasizing America's quest for a "liberal capitalist world order" avoids the simplistic and dogmatic analyses that this work reveals.

According to Bell, Wilson clothed America's special interests in terms of international principle and morality, viewing American society as on the verge of a social and economic crisis. Like many of his contemporaries Wilson saw the solution in terms of looking abroad, and when international complexities interfered with his vision of American needs and interests, he tried to define a "new system of relationships in order to 'make the world safe for democracy'" (p. 6). The new order would avoid internal factionalism and conflict by renewing opportunity provided by the growth of the United States, a nation of "the elect," in the world economy. The problem of understanding Wilson resides in an apparent disparity between his thought and his actions, but, as the author comments, moral principles are not policies.

Bell depicts Wilson's "New Freedom" as a "highly nationalistic coordinated campaign to expand American trade throughout the world . . ." (p. 41). He first became concerned with Latin America as the main area where he could apply his concepts—hence the interventions in Mexico and the Caribbean, and the genesis of Wilson's plan for a hemispheric pan-American pact. War in Europe caused the president to

project his concept to the world scene, envisaging a global community that would permit the United States to flourish everywhere. The author traverses well-known ground in relating the story of neutrality and the decision to enter the war upon the Allied side early in 1917, giving insufficient attention to Germany's motives in the U-boat campaign. His assertion that Wilson "moved to war almost automatically while still talking of peace" (p. 186) distorts a complex transitional period.

Even as Wilson took the nation into war, the image of the peace had begun to emerge in his thought: "American economic expansion required collective security—a primarily Anglo-American alliance against hungry rivals for markets and against revolutionary disruption and war" (p. 192). If these were his objectives, he achieved them at Versailles, the author concludes, and one may view the peace treaty and its failure ultimately as the failure of Wilsonian liberalism.

It is hard to find any real contribution to our knowledge of the period in this dogmatic volume. What it says has been better said elsewhere.

DANIEL M. SMITH  
University of Colorado,  
Boulder

ORVILLE H. BULLITT, editor. *For the President—Personal and Secret: Correspondence between Franklin D. Roosevelt and William C. Bullitt*. With an introduction by GEORGE F. KENNAN. Boston: Houghton Mifflin Company. 1972. Pp. xlvii, 655. \$12.50.

Researchers at Hyde Park denied access to parts of the Bullitt-Roosevelt correspondence may be interested now to find out what they missed. Some of these messages have appeared in whole or in part in *Foreign Relations* volumes for 1933-44, but much in this book, put together by Bullitt's brother from family papers, has not been published before. As first American ambassador to the Soviet Union, Bullitt entertained the president with lively vignettes. At a welcoming dinner in December 1933 he met Stalin, who offered to see Bullitt at once any time of the day or night. When he left Stalin took Bullitt's head in his two hands and gave him "a large kiss!" Bullitt added a curious compliment. "Like every real statesman

I have known," he told Roosevelt, Stalin has the "quality of being able to treat the most serious things with a joke and a twinkle in his eye. Lenin had that same quality. You have it."

Bullitt came to understand that the Russians lavished friendliness in 1933 only because they feared Japanese aggression. It took longer to realize that one did not easily understand Stalin or Kremlin politics. When S. M. Kirov was murdered the ambassador relayed the "highly confidential" news that it had been a crime of passion. The assassin Nikolaiev had killed Kirov because of his liaison with Nikolaiev's wife. Who could have realized in 1935 that Stalin was probably behind the Kirov assassination? Yet Bullitt's rush to report "the facts" suggests the level at which he early approached Soviet complexities.

Bullitt's correspondence is an odd mixture of intense idiosyncrasy and brilliant foresight. In June 1940, as ambassador to France, he demanded that Washington send submachine guns (he would pay for them himself) to protect Paris from the communist mob likely to appear when the French government fled. More often Bullitt's messages were prophetic. In a series of remarkable letters in 1943 Bullitt warned FDR that he did not understand Stalin's hope to dominate postwar Central and Eastern Europe. America ought to obtain promises from Stalin in exchange for aid to rebuild the Soviet Union. To make it more difficult for Stalin to renege, England and the United States should try to reach the eastern frontiers of Europe via Salonika and Constantinople before the Red Army. And if they did not? Europe would be divided.

There is no reply. Judging from this volume the president preferred conversation. If individually Roosevelt's answers seem lighthearted, in large numbers they are banal. "Pin a rose on Lenin when you attend the May first celebration. He is a great man because dead!" The level of FDR's correspondence rarely becomes more reflective.

By 1943 the president was no longer receptive to Bullitt or his ideas. Angered over Bullitt's role in forcing Sumner Welles from government—an imbroglio documented here—FDR refused his ambassador a position. Should Bullitt have been allowed to serve? George Kennan recalls in his introduction that

Bullitt told him "he had never subordinated his life to the needs of any other human being." And Bullitt's letters indicate that he was unlikely to see issues in other than black or white. Perhaps Bullitt would not have functioned well in the cabinet. Yet this correspondence suggests that few people in the early forties provided Roosevelt with a comparable range of insight and information.

BEATRICE FARNSWORTH  
Wells College

JERRE MANGIONE. *The Dream and the Deal: The Federal Writers' Project, 1935-1943*. Boston: Little, Brown and Company. 1972. Pp. xvi. 416. \$12.50.

In a photograph of the executive staff of the WPA Federal Writers' Project Jerre Mangione is seated at director Henry Alsberg's immediate right. Thus Mangione had, on that interesting venture of the New Deal into the arts, a seat with a view. Three decades later, after searching his own memory and the memories of other project members, reviewing the writings of and about the project, and going into the surviving records, he has come up with an account considerably more readable than most of the books the project itself produced. He has recaptured it all—the despair, the hope, the excitement. Like the repatriated writers of the 1930s recalling their nights and days in the Paris of the 1920s, Mangione and his former colleagues reminisce about the gallons of whisky drunk, who slept with whom, the Stalinist-Trotskyist fights on the big-city projects, the Dies Committee investigations, and all the confusions and uncertainties that were characteristic of the project.

Unlike the carefully planned Historical Records Survey the Writers' Project sort of grew, mostly out of the mind of Alsberg. It put jobless writers and persons in related fields to work producing guidebooks: national, regional, state, and local. Eventually there was a published guide for each state and for some of the cities.

As the depression went on, the dreamers dreamed of "portraying the nation in such an honest and effective way that it would help create a more noble standard of social behavior." Or as one of the writers later expressed it,

their goal was "to produce the Story of America. Not just history, not merely the politics, the economics, the village folklore, the literature, but the whole thing." All this called for books "that would have revealed the nation's soul more tellingly than the guidebooks." The project began collecting life stories, slave narratives, and folklore, and material on ethnic groups, place names, and other subjects, and prepared a few local histories. The result was the 150-volume "Life in America" series.

Meanwhile the state projects were having enough troubles with the guidebooks without getting into soul-revealing. A few of the cities that were the natural gathering places of artists had writers to spare (including plenty of prima donnas); but out in the provinces they were scarce. The state projects filled with persons whom the relief agencies certified as "writers." Few really were. The Idaho state director, Vardis Fisher, after looking over his staff and not finding anybody who could write, in ten months singlehandedly wrote 374 of the 405 pages of the Idaho guidebook. Only his secretary and his typist were of any real help. Idaho's guidebook was the first to be published. Ray Billington, who in addition to teaching directed the Massachusetts project, had little doubt that "a well-trained team of researchers could have produced the Massachusetts Guide with one fourth the number and less than one fourth the time."

The guidebooks are, deservedly, what the Writers' Project is best remembered for. (And Mangione gives due credit for their quality to a dedicated headquarters editor, Katherine Kellock.) Included in the *Dream* is a bibliography of the project's publications. In view of the thousands of persons employed—6,684 at the peak—and the more than seven years the project lasted, the list is not impressive. Mangione mentions the hoard of material that did not get published—much of which is now stored in the Library of Congress—and suggests that all such material ought to be located and inventoried. This should be done, just as the nearly forgotten unpublished products of the Historical Records Survey should be located and inventoried. A guess is that the HRS material will prove a historical find, and that the final judgment of almost all of the Writers'

Project unpublished material will be that it was not and never will be publishable.

If the Writers' Project, viewed in retrospect, comes close to qualifying as a well-intentioned boondoggle, it was not a scandalous waste of the taxpayers' money. It made nobody rich. It helped a number of people through some bad times; and if when they left most were no nearer to being writers than when they came, the experience did them little harm. Within a few years many of them were fighting or working in a war, the cost of which exceeded in one day the entire expenditure on the project.

Mangione concludes that "the experience of the Federal Writers' Project offers no clearly defined lessons for those who champion the cause of governmental subsidy of the arts." But the book supports, even though the author may not, W. H. Auden's contention that the WPA arts project was perhaps "one of the noblest and most absurd undertakings ever attempted by any state. Noblest because no other state has ever cared whether its artists as a group lived or died. . . . Yet absurd, because a state can only function bureaucratically and impersonally—it has to assume that every member of a class is equivalent or comparable to every other member—but, every artist, good or bad, is a member of a class of one. . . ."

There was in the 1930s some good writing, of which a little will last; but not much of it was by the Writers' Project. Writing such as young James Agee's *Let Us Now Praise Famous Men*, which tells more about the tenant farmers of the depression South than all the life stories gathered by the project and which will be read when the project's books are out of print and forgotten, is not to be found among the project's publications. Auden was right. A federal writers' project is likely to make the wrong assumptions and to function in the wrong way, whether under a Roosevelt or a Nixon or a future president of one's choice. The talented, creative, serious writer has to be free to write as he pleases; and for that the garret, however uncomfortable, is better than the federal office building.

LEONARD RAPPORT  
National Archives

GEORGE H. GALLUP. *The Gallup Poll: Public Opinion, 1935-1971*. Volume 1, 1935-1948;



volume 2, 1940-1958; volume 3, 1959-1971. New York: Random House, 1972. Pp. xlv, 777; 779-1584; 1585-2388. \$95.00 the set.

The three large volumes of this collection contain the findings of all of the published Gallup reports from the founding of the Gallup poll in October 1935 through December 1971. There are more than 7,000 reports in all. Each gives the survey and the question numbers, and the dates on which the interviewing was conducted. The questions appear exactly as they were asked, but, to save space, the editorial and interpretive material that accompanied the reports has been eliminated. Most questions were asked of a cross-section of the national population. But there are also reports of Gallup interviews with special groups—among them the county chairmen of the two major political parties, college students, and interviews with persons listed in *Who's Who in America*.

From the very first Gallup question—"Do you think expenditures by the Government for relief and recovery are too little, too great, or just about right?" (60 per cent thought they were "too great" in 1935)—to the last—"What woman that you have heard or read about, living today in any part of the world, do you admire the most?" (the answer in 1971: Mrs. Golda Meir), the pages chronicle both the changing and the not so changing attitudes of the American people on a wide range of economic, social, and political issues. All of the great moments are here: the correct Gallup forecast that President Roosevelt would be re-elected in 1936; the public's reactions to the marriage of the duke of Windsor and Mrs. Simpson; the changing American attitudes in 1939-41 toward possible involvement in World War II; the great embarrassment of 1948, when the Gallup poll elected President Dewey (the actual deviation of the final poll from the election results was *less* in 1948 than in the Democratic landslide of 1936); the emergence and sometimes the fading of public perceptions of such figures as Dwight D. Eisenhower, Estes Kefauver, John Foster Dulles, Sherman Adams, Adlai Stevenson, John Kennedy, Lyndon Johnson, George Romney, and Richard Nixon; and a series of final pre-election polls that were for the most part breathtakingly close to the actual vote in the years since 1952.

The impact of technological change on

American life is also reflected in the figures, from the mere 4 per cent of the public who reported watching any part of the national conventions on television in 1948, to the emergence of near universal public reactions to events, personalities, and issues as a result of seeing them on television just a few years later. And there are the changing social concerns, as new issues that were hardly mentioned in earlier years begin to emerge in response to the famous Gallup question: "What do you think is the most important problem facing this country today?"

Several aspects of the collection will be of special interest to those who want to use the data for scholarly work. The trend data, which capture changes in public attitudes in response to the same or similar questions asked repeatedly over time, are particularly valuable. To take one example, Dr. Gallup apparently first asked a sample of the American public whether they approved of the "18 year old vote" in 1939. The answer: 17 per cent yes, and 83 per cent no.

Then came World War II, when the argument was pressed that "if they are old enough to fight they are old enough to vote." The trend during the war and thereafter was as follows: (The three numbers after each date represent, in order of appearance, yes, no, and undecided or no opinion.) October-November 1942: 41 per cent, 53 per cent, 6 per cent; March 1943: 42, 52, 6; August 1943: 52, 42, 6; February 1947: 35, 60, 5; August 1951: 47, 49, 4; May-June 1953: 63, 31, 6; January-February 1954: 58, 34, 8; July 1965: 57, 39, 4; March 1967: 64, 38, 4. By 1970, when the nation was on the verge of moving to the eighteen-year-old vote, the percentages stood at 58 per cent yes, and 38 per cent no.

The data also provide revealing insights into the attitudinal context within which American political leaders have had to operate. On some issues public attitudes were severely limiting. On others, however, they were permissive. Before President Roosevelt publicly committed himself to an attempt to lead the United States into a new League of Nations, Dr. Gallup asked the following question in December 1912: "Should the Government take steps now, before the end of the war, to set up with our allies a world organization to maintain the fu-

ture peace of the world?" The response was 64 per cent yes, and 24 per cent no. (As one reads through the findings, one wonders what the effect of some of the Gallup questions was on the political leaders of the time.)

Sometimes the data also illuminate the interplay—and the differences—between attitudes in the mass public and the views of more elite groups in the population. In the spring of 1953 the Gallup poll found that 57 per cent of the general public did not have an opinion about Senator Joseph McCarthy of Wisconsin. And in the minority with an opinion, sentiment was about evenly divided between those who were favorable toward the Wisconsin senator and those who were unfavorable. (The figures: 57 per cent no opinion, 2 per cent neutral, 19 per cent favorable, and 22 per cent unfavorable.) In 1953 Dr. Gallup also interviewed a cross-section of persons listed in *Who's Who In America* about Senator McCarthy. Only 9 per cent of this group had no opinion, and the rest divided 28 per cent favorable, and 63 per cent unfavorable.

Some additions might be made when what we hope will be a new supplementary volume of this Gallup collection appears in a few years time. A series of tables summarizing the answers given over the years to certain major questions that have been asked repeatedly would be helpful. As it now stands, one must work through all three volumes to come up with a set of data such as the trend of public thinking since 1939 on the eighteen-year-old vote. To make comparisons easier, it would also be useful if the actual election percentages could appear beside the detailed listings of the final pre-election polls. And one could use a fuller index with more cross-references, to facilitate the recapture of specific questions.

But these are minor quibbles. The collection is a major landmark in the history and measurement of American public opinion. It provides fascinating, and at times surprising reading. It will encourage many to delve more deeply into the basic reports themselves (complete data card decks for all of the Gallup reports released since 1935 are on file at the Roper Public Opinion Research Center in Williamstown, Massachusetts). And scholars who work with pre-1935 materials will envy their colleagues of the post-Gallup era who have

these reports as one more data source in their efforts to recapture and analyze the American past.

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*Foreign Relations of the United States, 1947. Volume 5, The Near East and Africa.* (Department of State Publication 8592.) Washington: Government Printing Office, 1971. Pp. ix, 1377. \$6.50.

The latest volume of *Foreign Relations of the United States* to deal with the Middle East, which covers the very critical year 1947, is a fitting illustration, not merely of the enduring relationship of the United States with that troubled area, but of the wide-ranging character of American policy and interest. The volume centers, appropriately enough, on developments along the northern tier of Greece, Turkey, and Iran and on the events in Palestine that were to lead, in the end, to its partition.

One notes at the very outset the regional character of American policy in the Truman Doctrine of March 12, 1947, for assistance to Greece and Turkey; the "Pentagon Talks of 1947" between the United States and the United Kingdom concerning the Middle East and the eastern Mediterranean; American participation in the development of Middle Eastern petroleum resources; the American interest in Communist and Nationalist activities in North Africa and the confusion in American policy relative thereto; the American interest in the proposals of King Abdullah of Transjordan for a greater Syria; and the American interest in resolving the dispute between Afghanistan and Iran regarding distribution of waters of the Helmand River.

The volume covers such a wide area that it is possible to provide only a sketch of the problems covered. Here one will find, for example, ample documentation (pp. 761-814) on the American interest in the Anglo-Egyptian negotiations for revision of the treaty of August 26, 1936, which still governed Anglo-Egyptian relations, and the question of possible Egyptian restriction of the right of transit through the Suez Canal. The latter problem, of course, persisted in one form or another throughout the post-World War II period.

The problem of aid to Greece and Turkey

covers some 484 pages. The research student is now able to get at the essential elements in the development of American policy as symbolized in the Truman Doctrine. A good section (pp. 816-89) of the documentation deals with the development of the Greek question in the United Nations during 1947 and the investigation of incidents along the northern frontiers of Greece and the activities of Greek guerrillas. The documentation supplements the more detailed records of the United Nations, though more evidence on this subject would have been useful. The section on Iran (pp. 890-998) is largely devoted to the Iranian-Soviet impasse over the agreement of April 1946 concerning the exploitation of oil resources in the northern part of Iran.

Especially noteworthy in this volume are documents regarding the Palestine problem, which take up some 345 pages (pp. 999-1328). These well-selected documents tell something of the story of the involvement of the United States in the Arab-Zionist conflict over the future status of Palestine, a conflict that culminated in the partition resolution of November 29, 1947. Among the interesting documents in the published materials is that of Loy W. Henderson, then director of the Office of Near Eastern and African Affairs, Department of State, of September 22, 1947 (pp. 1153-59), in which he indicated that he could not recommend the partition of Palestine as being in the national interest of the United States. As Mr. Henderson put the problem: "An advocacy on our part of any plan providing for the partitioning of Palestine or the establishment in Palestine of a Jewish state would be certain to undermine our relations with the Arab, and to a lesser extent with the Moslem, world at a time when the Western World needs the friendship and cooperation of the Arabs and other Moslems."

The Historical Office of the Department of State is to be congratulated on its selection and editing of documents in this volume dealing with developments in the Middle East since World War II. Complaint may be made, as indeed it is from time to time, as to the lag between events and the publication of the documents—some 25 to 26 years. While there are omissions, granted the tons of materials that come into the department, there would appear

little doubt that these volumes constitute a valid official record of American policy. All students of American policy and interest in the Middle East must now study the record of 1947.

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#### CANADA

SAMUEL ELIOT MORISON. *Samuel de Champlain: Father of New France*. (Atlantic Monthly Press Book.) Boston: Little, Brown and Company, 1972. Pp. xix, 299. \$10.00.

Samuel Eliot Morison has written some excellent books. This is not one of them. In the preface he states, "here is my best effort to honor one of the greatest pioneers, explorers and colonists of all time." In short, the work belongs more in the realm of hagiography than critical biography.

Champlain was certainly an outstanding figure of his time. He was a highly proficient navigator, and Morison deals with that aspect of his career in exemplary fashion. His chief claim to fame, however, was that for a quarter of a century he governed the commercial enterprise at Quebec that eventually became a thriving colony, but at his death in 1635 the struggling settlers numbered only some 150. Thus to claim, in the concluding sentence, that "no other European colony in America is so much the lengthened shadow of one man as Canada is of the valiant, wise and virtuous Samuel de Champlain, Xaintongeois," is to exaggerate.

No real attempt is made to put Champlain in a proper perspective. He always appears much larger than life. The involved background of forces, both in America and Europe, that controlled Champlain's actions is treated superficially, and some aspects are misrepresented. The abundance of factual errors, not all of them trivial, make it plain that the author lacks a proper familiarity with the history of the period, and of this particular colonial endeavor. We are told (p. 160) that the Iroquois-Huron war "dragged on for as long as the colonial period lasted." It ended in 1650. The old, bankrupt notion is resurrected that New France would have thrived had Huguenots been encouraged to settle there. The value of the French *livre* is equated throughout at five to

the dollar "in gold," whatever that may mean, which results in highly misleading comparisons. The context alone should have indicated that the *charrue*, so eagerly awaited by the first settlers, was a plow and not a "two-wheeled cart." The misspelling of other simple French words makes the author's unfamiliarity with the language all too obvious.

The literary style is, to say the least, racy. The reader is swept along by the swift flowing, narrative account of events, but is too frequently jarred by colloquialisms. Things are "all bunged up"; Indians "hit the war path" and are "rubbed out"; portraits are "phonies"; Henry IV "beefed up his company"; a murderer "continued to be a bad actor."

To sum up—there is nothing new in this book, except some of the mistakes.

W. J. ECCLES

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*Mike: The Memoirs of the Right Honourable Lester B. Pearson. Volume 1, 1897-1948. [New York:] Quadrangle Books, 1972. Pp. x, 301. \$12.50.*

This book is the first of a projected three volumes, but Pearson died December 27, 1972, while this review was being written. He would be the first to deny the proposition, *de mortuis nil nisi bonum*.

Mike Pearson was born and bred a Canadian and came from an Ontario Methodist parsonage. His world view in later years always retained that belief in the perfectibility of the human condition that is the eighteenth-century inheritance of the Methodists. After a familiar Toronto-Oxford education he became a history professor at the University of Toronto. But he was not an intellectual, and when he was given the opportunity to join the Canadian Department of External Affairs he did not refuse. He served honorably and well, and from 1942 to 1946 was at the Canadian embassy in Washington, becoming ambassador in 1944. He was prime minister of Canada from 1963 to 1968.

There is a good deal of Pearson in this book: his utter lack of pomposity, his sly sense of humor, his charming quality of being not infrequently a lovable loser, his understandable and unobtrusive pride in his own achievements. It is pre-eminently a book for Ca-

nadians, who will enjoy it, and it will puzzle Americans, who may wonder why Canadians published it at all. It is old-fashioned, prosy, and good-natured, like a leisurely tea in a spacious and sunlit garden in a quiet Ontario parsonage, taking the small with the large almost as if they were equal, very like Pearson, who took people as they came. He will often tell a story against himself—how, for example, he lost the university football cup for his college by dropping the football when he was behind his own goal line. One cannot but help enjoying his honesty. There frequently comes to the surface a fine sense of irony. Once in Washington during the war the Pearsons had a royal duchess to dinner at the Canadian embassy, and James the butler was serving a white wine, a Liebfraumilch. The royal dame asked the name of the wine. "Very conscious of the presence of Royalty and the impropriety of using German words in wartime, especially in such august company, James did a quick and literal translation: 'Milk of the Virgin, your Royal Highness.'" Or on Mackenzie King, the Canadian prime minister: "You would think of him on these occasions as merely a kind, simple old man and you could hardly be more mistaken."

When all is said and done, it is very much a Canadian book. Reviews of it in Canada have been generally favorable, and it is on best seller lists. But for Americans, this book is, in repeating Pearson's remark when he succeeded in making some small corrections to Churchill's Fulton, Missouri, speech, "a tiny footnote to contemporary history."

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#### LATIN AMERICA

M. S. AL'PEROVICH and L. IU. SLEZKIN. *Novaia istoriia stran Latinskoï Ameriki* [A New History of the Countries of Latin America]. Moscow: Izdatel'stvo "Vysshiaia Shkola." 1970. Pp. 382.

M. S. AL'PEROVICH. *Ispanskaia Amerika v bor'be za nezavisimost'* [Spanish America in the Struggle for Independence]. Moscow: Izdatel'stvo "Nauka." 1971. Pp. 221.

Soviet and most of U.S. historiography on Latin America is founded on radically different axioms, the one on dialectical materialism, the

other on a variety of economic, social, and cultural assumptions. During the Stalinist years Soviet historiography was shackled by dogma that lumped all Latin Americans, past and present, into the "counter-revolutionary camp." Since the mid-fifties there has been a return to Leninist analysis, which is far more subtle, drawing many more distinctions within the "bourgeois" world, and great strides have been made in both the quantity and quality of scholarship. These two works are a continuation of this improvement.

Al'perovich is the dean of Soviet historians specializing in Latin America, and for fifteen years he has written or edited numerous works on the wars of independence, the national period, and the Mexican revolution. Slezkina is a specialist on the Cuban revolution who more recently has concentrated on Brazil. Their coauthorship of *Novaya istoriia* is designed to meet the need in the USSR for a college textbook on Latin American history from the sixteenth century to the end of World War I, to help fill the gap in "Marxist scientific and popular literature" on the subject. It is not a complete textbook because of space limitations, as the authors state at the outset. The colonial period is reviewed briefly, primarily as an introduction to the wars of independence to which a third of the book is devoted. The rest concentrates on the subsequent histories of Argentina, Brazil, Chile, Cuba, and Mexico, while the other Latin American countries are discussed only in their relation to important historical events.

Latin American history is presented in terms of class struggle, and those areas of the eighteenth and nineteenth centuries that smacked of twentieth-century issues are highlighted. The Haitian revolution of the 1790s and early 1800s receives more than ordinary attention for a textbook and is credited with having influenced slaves in the Hispanic colonies and the United States to rebel. The Petion constitution of 1816 is cited as having prohibited foreign ownership of property in Haiti and thereby protecting the country's new-found independence. In actuality the constitution prohibited any whites from ownership; after twelve months' residence foreign blacks had all the rights of a Haitian citizen including land ownership. Nevertheless, the point is interesting

and deserves further investigation. The War of the Pacific also receives special attention, and the role of British capital in it is emphasized. The British had investments in both Peru and Chile, but in Peru they were of low value because of their exclusion from nitrate mining and the continued financial crisis of the government. In Chile, on the other hand, there was a financially solvent government that gave the British free rein in the mining of nitrates. When a showdown came over the mineral rich areas of Tacna, Arica, and Tarapaca the British strongly supported the eventually victorious Chileans and thus became primary beneficiaries of the war.

*Ispanskaia Amerika* is another addition to the growing Russian bibliography on the wars of independence, a favorite subject of Soviet historians. The independence movements in Latin American colonies are seen as having many aspects of a bourgeois revolution, in that in most areas they ended a number of forms of feudalism such as slavery, compulsory work by Indians, royal monopolies, and prohibitions and regulations that inhibited economic growth. More favorable conditions were created for establishing Latin American capitalism and drawing the region into the world economy. The struggle with Spain temporarily united all classes: Negroes, Indian peasants, petite urban bourgeoisie, and landowners. The main burden of the struggle was carried by the masses who did the majority of the fighting, but because of the weakness of the petite bourgeoisie and the incapacity of the peasants to lead, the movement for the most part was led by Creole landowners. Thus the masses were unable to put forth their own class requirements. Hidalgo's revolt in Mexico and Francia's regime in Paraguay were in part exceptions in that fundamental questions about landownership were raised. The Creole landowners feared the possibility that the independence movement would become a social revolution and restrained any activity in that direction.

Both books are recommended for the insights they provide into Leninist analysis of Latin American history, but a serious drawback is that neither has reference footnotes. While this is understandable for a textbook, it is less so for *Ispanskaia Amerika*. It is important that the Soviet historians footnote their



works on every possible occasion, because their different interpretations of particular events or documents inspire Western scholars to consult the original sources to see if indeed their views are more valid than conventional Western interpretations.

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[FIDEL CASTRO.] *Fidel in Chile: A Symbolic Meeting between Two Historical Processes. Selected Speeches of Major Fidel Castro during His Visit to Chile, November 1971.* New York: International Publishers. 1972. Pp. 234. Cloth \$7.50, paper \$2.65.

JAIME SUCHLICKI, edited and with an introduction by. *Cuba, Castro, and Revolution.* Coral Gables, Fla.: University of Miami Press. 1972. Pp. xii, 250. \$7.95.

LOWRY NELSON. *Cuba: The Measure of a Revolution.* Minneapolis: University of Minnesota Press. 1972. Pp. xiii, 242. \$10.00.

MAURICE HALPERIN. *The Rise and Decline of Fidel Castro: An Essay in Contemporary History.* Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 380. \$12.95.

The flood of books on Cuba is unceasing. By now, probably fortunately, the early torrent of facile, lightly rooted, and often misinformed journalistic accounts has tapered off, and many of the new studies are scholarly products of permanent value. The four volumes under consideration in this review are all valuable, and in three cases will deserve a lasting place on the much-more-than-five-foot-shelf of sober studies devoted to Cuba.

The one that can be treated most briefly is *Fidel in Chile*, ambitiously subtitled with a quote from Castro: "A symbolic meeting between two historical processes." This slender volume contains a selection of the many speeches, sometimes abbreviated, made by Castro during his whirlwind three week visit in Chile in late 1971. No editor is identified, and the editorial treatment in some places is more lyrical and less tight than might be desired. The freewheeling and exuberant Castro was at his oratorical best in many of his speeches in Chile and in exchanges with members of his audiences. Many of the questions asked of Castro sound "planted."

The collection of speeches is pure Castro,

filtered through rosy-tinted editorial glasses, but as source material the book is extremely useful.

The volume edited by Jaime Suchlicki, who also contributes an excellent synthesizing introduction, is a collection of essays by authorities on various aspects of the Cuban scene. *Cuba, Castro, and Revolution*, as a title, covers a broad spectrum but the contents are as varied as the title would suggest. The essays were written specifically for this collection and in most cases report on the findings of the respective authors' ongoing research projects.

W. Raymond Duncan discusses Cuban nationalism, Lowry Nelson reports on changes in the island's social structure, Sergio Roca and Roberto Hernández deal with economic problems, Gemma Del Duca treats the Cuban cultural scene, former ambassador Foy Kohler writes of Cuba and the Latin American-Soviet problem, Leon Goure and Julian Weinkle continue the Russian aspect with an essay on "the growing integration" between Cuba and the USSR, and Michael Kline concludes the volume by writing about Castro's challenge to Latin American communism.

It is not to denigrate the other essays, all of which were competently done, to record that this reviewer felt that the meatiest ones were those by Duncan, Nelson, Roca and Hernández, and Goure and Weinkle. Almost all of the seven essays reveal a high level of scholarship, and all but one are carefully documented.

Lowry Nelson's *Cuba: The Measure of a Revolution* is a sober and altogether excellent assessment of the political, social, and economic consequences of the convulsion that has been Cuba during the almost decade and a half just past. Nelson, author of the landmark *Rural Cuba* (1950), could not repeat his earlier field work in Cuba inasmuch as the Cuban government would not grant him a visa, but his consultation of sources in the United States, both documentary and personal, was extremely thorough, and the result is an entirely scholarly and well-documented study.

The study will not win its author any medals from the current Cuban government. Its net effect is to upset many of the common assumptions about the revolutionary regime. All revolutions, Nelson properly points out, are matters

of controversy and cannot be put in perspective for many years or even generations. The Cuban revolution is the more controversial both because of its extremes and its intricate involvement in the power plays and feints of the world's two great states.

The main divisions of the study are essentially historical, economic, and sociological. The author on balance gives full credit for moral reforms, educational achievements, and other accomplishments, but he also points up the fantastic downhill slide of the Cuban economy, the quixotic dangers inherent in keeping the revolution in Castro's own pocket, i.e., failing to institutionalize it, and the anesthetizing of all critical reaction to political judgments from the Cuban masses.

Halperin's *Rise and Decline of Fidel Castro* is based on more firsthand Cuban experience than the books of any of the other authors (*Fidel in Chile* of course excepted). He spent nearly six years, 1962-68, at the University of Havana. The result is more of a feel for the nuances and subtle byplays in Cuban politics than the other studies here reviewed show. The author's ample documentation is drawn largely from Cuban sources, *Hoy*, *Revolución*, *Granma*, and numerous books. This is both an advantage and a liability. Many *norteamericano* writers have not had access to such long runs of the periodicals, and yet it would be the ultimate in fallacies to believe those sources are objective or unbiased.

Halperin's treatment is far ranging, especially with regard to foreign policy involvements. He is no Castrophile, but his basic sympathies with the Cuban revolution do come through loud and clear. Yet his biases do not prevent his seeing the decline as well as the rise of the charismatic Castro. Though the chief emphasis is on the period through 1964 (a second volume is promised) there are flashbacks and glances ahead in such fashion that the organization is at times chronologically confusing.

The study is a valuable one, especially by virtue of its full-dress treatment of the complex, delicate, and often contradictory maneuvering of the Castro regime in Latin American and world politics.

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WILLIAM B. TAYLOR. *Landlord and Peasant in Colonial Oaxaca*. Stanford: Stanford University Press, 1972. Pp. x, 287. \$10.00.

A work in Latin American colonial history dealing with a province remote from any capital, covering the whole colonial period, and synthesizing an aspect of social-economic life from a broad, scattered documentation, can hardly fail to make an important contribution to the field. Taylor's book, thoroughly and judiciously done, belongs to a second or post-Chevalier wave of agrarian studies: abandoning Chevalier's eclecticism, it concentrates on the single region of the valley of Oaxaca, with relentless attention to land tenure, somewhat like Borde and Góngora's *Valle del Puangué*. The study also, Taylor being a student of Charles Gibson's, in some respects comes close to being a corporate history of the Indians of the region. The social or general history that has dominated scholarly writing in the field for some five years is not without its influence either, since Taylor's book goes far beyond the narrow restriction to land that made the Borde and Góngora study so relatively arid, to become in many ways a general regional history. Whereas most books have a derivative, often outmoded *mise en scène*, Taylor's "The Setting" is a meaty chapter of largely new social and economic information.

Countless maps and tables give the reader an overall picture. A more basic value, however, attaches to the four central chapters, which (together with the appendixes) provide not merely aggregate information, but an exhaustive series of reconstructions of individual entities and organisms through time—chieftaincies (*cacicazgos*), haciendas, villages, and religious institutions, at least in landholding aspects. Like Gibson's *Aztecs* or a good novel, this book contains the germ of interpretations and patterns not fully articulated by the author and which will not be immediately grasped by the reader, but which will make the work productive of new insights for many years.

The Indians retained adequate valley land, indeed most of it, through the whole period; chieftains kept their identities and estates; Spanish haciendas were neither all-encompassing nor generally stable in ownership; ecclesiastical owners sold land as others did. Such is

the main and incontestable intended burden of the book.

Questions remain. Is "land tenure" really a good approach to history? "Land was the main economic foundation of the social order," we are told. It is hard to imagine that this is the same province that Brian Hamnett recently discussed predominantly in terms of its principal economic resource, the cochineal trade. The word "cochineal" appears in Taylor's study about ten times, and he himself says that cochineal was produced mainly on otherwise worthless land.

In the ongoing discussion concerning "debt peonage" Taylor does follow the trend in playing down the significance of that alleged institution, but he accepts the category, making the now hardly tenable assumption that peonage was based on debt if debt was present at all. He reduces the distinctions between temporary and permanent labor to chronological dimensions, not allowing for or describing temporary labor after the demise of the formal *repatriamiento*.

When characterizing the Oaxacan situation, Taylor tends to speak of "southern" as opposed to north or central Mexico. This type of categorization will not do if we are ever to understand the evolution of Spanish American estates; we cannot go on investigating northern and southern variants in some twenty countries. We must speak in terms of general principles or tendencies and structural characteristics. Taylor knows enough to do this, but in his maddening caution refuses to make a statement about the clear causal links. Near a large capital city or a major economic asset like silver, there will be large numbers of Spaniards and consequently a market, estates will quickly consolidate, and the Indians (if any) will lose land, population, and aspects of organization. Where, as in Oaxaca, those things are lacking, estates will be slow to consolidate and Indians will retain their position longer.

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JOHN PATRICK BELL. *Crisis in Costa Rica: The 1948 Revolution*. (Latin American Monographs, number 24. Institute of Latin American Studies, University of Texas at Austin.)

Austin: University of Texas Press, for the Institute. 1971. Pp. xiii, 192. \$7.00.

This monograph is a solid contribution to Central American historiography and to the story of Latin America's place in the cold war. It analyzes the Costa Rican revolution of 1948 that brought José Figueres to hemispheric prominence as an anticommunist advocate of political change.

The author interprets this revolution as the culmination of social tensions that had been mounting in Costa Rica since 1940. The success of late nineteenth-century agriculture in Costa Rica had produced an elite class that by 1940 had become an oligarchy unable to face needed reforms. When President Rafael Calderón Guardia (1940-44) introduced such reforms to head off social violence, the elite viewed him as a radical and he became very controversial. His reputation was further tarnished when he imposed Teodoro Picado as his successor (1944-48) through electoral fraud. The result was a coalescence of diverse dissenting groups into the Opposition. José Figueres used this coalition as an instrument of revolution in 1948. He also used the nebulous Caribbean Legion and the growing hostility to communism in the United States.

Figueres, the son of a Catalan immigrant, is pictured as a loner-critic who became a revolutionary *caudillo civil* through a Machiavellian sense of purpose and timing. If there was any quixotic element in Figueres's personality and motivation, it does not appear here.

Bell's work demonstrates both the advantages and difficulties of the topical approach to history. Any of the major issues of Costa Rican politics in the 1940s (the social question, communism, corruption, conspiracy, or the electoral question) can be pursued quickly and in detail. But if one seeks a straightforward account of Costa Rican history in that decade, culminating in the revolution of 1948, he will be frustrated. For example, while the author refers early and often to the political importance of the fraudulent election of 1944, the reader remains uncertain and thus unpersuaded about it until pages 100 and 112-13. This antinarrative effect and the lack of a map or bibliographical essay or of citations to any of the author's two hundred interviews with principals, combine to create an unnecessary

air of abstraction. Citation of a wide range of sources and a good index save the day. This useful book is for specialists.

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ALAN H. ADAMSON. *Sugar without Slaves: The Political Economy of British Guiana, 1838-1904*. (Caribbean Series, 13.) New Haven: Yale University Press, 1972. Pp. ix, 315. \$12.50.

Professor Alan H. Adamson's main task in this book is to explain how the hegemony of sugar came about and to describe how it affected Guyanese society in the nineteenth century. With the emancipation of the slaves in 1838, the survival of sugar was threatened both from within and without. Planters had to contend with a rapidly forming society of peasant villagers who could not be relied upon to supply combined and constant wage labor to the plantations. Loss of labor was compounded by a series of external threats, including Britain's move to a free-trade policy, competition from the slave-grown sugar of Cuba and Brazil, and the European beet sugar industry.

Adamson's account of the survival of sugar, the plantation economy, and the plantocracy is most convincing. The planters' economic power was buttressed by their domination of the Guyanese government and support from the West India Committee in London. Political power was used for a variety of purposes. Village settlements of ex-slaves were restricted by means of high prices placed on crown lands and heavy tax levies. After growing for a time, the villages stagnated and their inhabitants found it necessary to seek wage employment on the plantations.

Between 1835 and 1918 a total of 341,491 immigrants arrived in British Guiana, 236,205 of whom were Indian. A system of state-controlled and state-conducted immigration led to a continuous influx of laborers who were indentured for a period of approximately five years. Stringent controls governed work, wages, and freedom of movement. Immigration was financed, in part, from public revenues that came chiefly from indirect taxes. "These high indirect taxes," writes Adamson, "were in effect a subsidy to the working capital of the planters by

the nonplanting part of the community" (p. 244).

Pressure to scale down costs in relation to prices led to the transformation of the Guyanese sugar industry and water-control system. Highly mechanized plantations were linked to one another and to trading firms in vertically integrated structures having joint-stock organization and a strong metropolitan orientation. By means of wage reduction, economies of scale, and subsidies a handful of giant concerns managed to weather the crisis of the late nineteenth century. But sugar monoculture exacted high social costs, since it drove out nonsugar industries, made the colonial economy vulnerable to world market conditions, narrowed the range of trading partners, worsened the terms of trade, contributed to capital starvation, and resulted in commercial isolation from regional neighbors.

The strength of Adamson's book lies in its balanced use of literary evidence and statistical data taken from Colonial Office and Parliamentary Papers, the care with which the study is organized, and, above all, the high standards of economic analysis and historical interpretation which are consistently maintained. It is, however, somewhat disappointing to find so few references to, and no index items for, the British sugar colonies of Jamaica and Trinidad, which had many experiences in common with Guyana. Comparative study, for example, might have revealed why the peasantry of Jamaica increased by natural means, while that of Guyana failed to reproduce their numbers. These mild criticisms notwithstanding, Professor Adamson has given us an outstanding study of the transformation of a tropical export economy in a century of momentous change.

RICHARD B. SHERIDAN  
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J. VALERIE FIFER. *Bolivia: Land, Location, and Politics since 1825*. (Cambridge Latin American Studies 13.) New York: Cambridge University Press, 1972. Pp. xiii, 301. \$23.50.

The recognized frontiers of the Republic of Bolivia today include only about half of the area that the country claimed at the time it obtained its independence in 1825. This book is a thorough account of just how Bolivia's diminution took place.

As the author, principal lecturer in geography at Goldsmiths' College of the University of London, points out, Bolivia was based on the colonial *audiencia* or presidency of Charcas, also known as Upper Peru. This unit had been shifted from the viceroyalty of Peru to that of Buenos Aires, when the latter was established in 1776. It appeared to many, including Bolívar, at the time of independence that Charcas had little chance of maintaining a separate existence.

The Creole leaders who insisted on Bolivian independence did not listen to these fears, which in any case proved exaggerated. Although Professor Fifer notes all this, she might have indicated a bit more about why her neighbors did not partition Bolivia, a possibility that has even been seriously discussed in recent years.

Bolivia had problems on all of her frontiers. The only neighbor from which it was to gain any territory was Argentina, who offered no opposition to the decision of leaders of Tarija to shift from Argentina to Bolivia soon after independence. Yet even on this southern border, Bolivia was not able to make effective all of its other territorial claims against Argentina.

Probably most serious was the problem in the west. Bolivia emerged from the colonial period with a small wedge of territory bordering on the Pacific. This area, however, was largely desert and included no significant population center. Professor Fifer traces the efforts of the Bolivians to establish a port in the area, its attempts to convince Peru to transfer Arica (the natural outlet to the sea of Upper Peru in the colonial period), and the conflict with Chile that culminated in the War of the Pacific (1879–83) during which that country seized all Bolivian coastal territory. Bolivia's final loss of its seacoast was provoked by the discovery of rich nitrate fields there.

Rubber served the same function along the

Bolivian-Brazilian frontier as nitrates did on the Bolivian-Chilean border. After extended attempts to make its sovereignty effective over the Acre region in the Amazon area, this controversy was also resolved by armed conflict. Brazilian settlers revolted, first setting up the Republic of Acre and then successfully asking for annexation by Brazil. United States readers will find reminiscences of Texas here. In the same general area, Bolivia also had conflicts with Peru. These were, however, solved by peaceful negotiation—Bolivia losing claimed territory at the conference table rather than by force of arms.

Militarily the most serious conflict was that with Paraguay, in the east and southeast. From the time of its establishment, Bolivia tried unsuccessfully to establish an effective presence along considerable reaches of the Paraguay River and its tributaries, thus giving it access to the Rio de la Plata exit to the sea. A century after obtaining its independence, Bolivia's efforts caused a military conflict with Paraguay, in the disastrous Chaco War of 1932–35. Bolivia was ignominiously and decisively defeated. An interesting question, which Professor Fifer does not raise, is why the Chaco conflict left no spirit of "revanchism" in Bolivia. This contrasts with the aftermath of the War of the Pacific, which, as the author points out, left a still-persisting aspiration to regain a Pacific port.

Professor Fifer expertly traces all of Bolivia's frontier problems. Her prose is not scintillating, although it is adequate. The book is generously provided with maps and pictures. This volume leaves the reader well informed about Bolivia's long-standing frontier problems.

ROBERT J. ALEXANDER  
*Rutgers University,*  
*New Brunswick*



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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

*The following exchange of letters has been received in connection with the publication of Hans W. Gatzke's review article, "Hitler and Psychohistory," AHR, 78 (1973): 394-401.*

TO THE EDITOR:

Since I contributed a foreword to my brother Walter's book, *The Mind of Adolf Hitler*, I hope you will permit me to make a few observations on the lengthy analysis and criticism by Professor Hans Gatzke. In the first place it astonishes me that a professional historical journal should devote so many valuable pages to a book that is not history at all. My brother is not a historian and had no intention of writing history. His book is what it professes to be: a psychological study of a then living

person, based on admittedly fragmentary evidence and, from the very nature of the project, bound to rely in part on conjecture and clinical experience. I myself, though a historian, had almost nothing to do with the project. Neither had any of the members of the Research and Analysis Branch of the OSS, of which I was the chief. Apart from the pressures under which the entire agency was laboring during the last years of the war, it was hardly likely that a report, being secretly prepared for General Donovan in another branch of the OSS, should have been opened for general discussion. Nor is it clear to me what contribution even the ablest of the historians in the Research and Analysis Branch might have been expected to make to a psychological evaluation.

Once he became convinced that his report was not being effectively used, my brother lost interest in it. For twenty-five years he made no effort to have it declassified. When finally persuaded to have it published, he steadfastly refused to do so if revisions or updates were to be required. I think I can say that he had only a mild interest in its publication and never anticipated the widespread interest it evoked.

One of the strangest features of Professor Gatzke's critique is his insistence that the book is not what it purports to be: an original wartime report or "historical document." So far as I can discover, he is saying that only a first draft, however hurried and crude, has authenticity, and that changes, even editorial changes made in the interest of clarity, destroy its character as a historical document. This is rather novel doctrine, considering that historical documents are continually being pub-

lished in various forms: in selection, in translation, in modernized language and style, etc. So much is certain: that the first draft of the report, which eventually found its way into the National Archives, was superseded by the revised official, printed OSS report of 1943, from which the present book is taken. Why this official document did not reach the National Archives is anyone's guess, but will hardly amaze those who have had long experience in government work. It is to be hoped that Professor Gatzke is not so enamored of first drafts that in the case of his own writings he sends them directly to the printer. I have read most if not all of his published works and would say that they reveal careful, considered editorial revisions.

By referring to many "major and minor changes" made by my brother or by the publisher in the course of preparing the report and the book for publication, Professor Gatzke leaves the reader with the impression that he (the reader) has been made the victim of a hoax: that he is not getting the true version of what my brother wrote in 1943, but a doctored account. Actually most of the changes made at any time are of a piddling nature. Nowhere has Professor Gatzke or anyone else demonstrated that important changes were made either in substance or in interpretation.

It is hard, in perusing Professor Gatzke's critique, to escape the impression that it is designed to discredit the book in one way or another. Those of us who, with some reluctance, decided to publish a document already over twenty-five years old did so in all good faith. We still regard it as an interesting and important contribution; we are gratified by the interest it has aroused; and we resent the suggestion of intentional misrepresentation and particularly the imputation that there is anything even remotely fraudulent about *The Mind of Adolf Hitler*.

WILLIAM L. LANGER  
Cambridge, Massachusetts

#### TO THE EDITOR:

I was honored to have your journal publish Hans Gatzke's lengthy review article, which was primarily an attempt to evaluate my re-

cent publication, *The Mind of Adolf Hitler*. Inasmuch as Mr. Gatzke has raised questions concerning the book's validity and integrity I would appreciate this opportunity of replying to his allegations and insinuations.

Mr. Gatzke has failed to realize that my book is not a psychohistorical study and never pretended to be. It should, therefore, not be evaluated on this basis. Any similarities that may exist are superficial and coincidental. *The Mind of Adolf Hitler* was the product of a crash psychological program designed to fill an urgent need at a critical stage of the war. A psychohistorical study to fill this need would undoubtedly have required years of research. The war could not wait. It was probably for this reason that General Donovan did not assign the task to the able historians on his staff. Instead he turned to psychoanalysts in the hope that with their training and experience they might devise techniques for producing a pragmatic profile of our adversary in a relatively short period of time. This was a novel approach. In order that the reader might not be misled into thinking that he was getting anything more than a tentative and conjectural reconstruction of Hitler's character, I devoted considerable space in the introduction to a description of the new techniques that were improvised to screen the mass of unreliable and contradictory information and the modes of evaluating its significance in terms of the total personality. It should be obvious, therefore, that our study was limited to a consideration of probabilities to which traditional historical research procedures are wholly inappropriate. Nevertheless, Mr. Gatzke as well as some other historians have failed to grasp this important distinction and attempt to evaluate the one in terms of the other. If historians are going to concern themselves with psychological studies of this type it would, in my opinion, be far more fruitful for them to confine their evaluative efforts to items such as: how well did this novel approach fill the need for which it was designed? how accurate were the basic conclusions in the light of subsequent historical investigations? can these new methods be improved and incorporated more advantageously in a psychohistorical approach to an understanding of the past?

Mr. Gatzke raises the question of the rela-

tionship between *The Mind of Adolf Hitler* and a report written by Dr. Henry A. Murray. Had he read my introduction with the same diligence he has shown in comparing a quotation taken from an American translation of *Mein Kampf* with that found in a German edition I do not believe that he would need much further clarification. I stated very clearly that one of the collaborators, after reading all of the raw material (the "Source-Book"), found that he would not be able to take part in our joint evaluative meetings to be held in New York City. Since Dr. Murray was at Harvard at that time and the other two collaborators lived in New York it would seem rather obvious who the defector was. I also pointed out "that he promised, however, to write down his views and conclusions and submit them to us for our consideration." Gatzke then completely ignores the next sentence which reads: "Unfortunately, not a word was ever received from him." Under these circumstances it should be perfectly clear that Dr. Murray did write down his views and conclusions, but instead of submitting them to us, as promised, he chose to divert them into an unrelated channel. In view of the fact that we are both psychoanalysts, working with the same directives from identical source material at exactly the same time, it should not be too surprising that the language used and the conclusions reached are not unlike each other, as Gatzke observes. Some objective investigators might even conclude from these similarities that there must be more to this psychoanalytic approach than meets the eye.

Even more disturbing are Mr. Gatzke's assertions that the book is neither "the secret psychological report written in 1943" that it purports to be, nor does it qualify as a "historical document." The pertinent facts are quite simple although subsequent events have provided room for confusion. As set forth in the introduction, pressure of time forced me to submit the first draft of my report to the OSS for consideration. It was favorably received and deemed worthy of wider distribution under OSS auspices. Early in the fall of 1943 my manuscript was turned over to the printer who promptly set up the galleys. I was asked to do the proofreading and make whatever corrections were necessary. Incredible as it

may now seem the galleys afforded me the first opportunity to read what I had written. I was not greatly impressed. There were countless omissions, transpositions, and misspellings, as well as unclear passages, irrelevant statements, repetitions, and the like. To this day I do not know whether most of these were the result of my hasty writing or the hasty typesetting of the printer. At the time it made no difference. We were in the process of preparing an official document for distribution, and since I was the author it was my duty to make every effort to state the substance of my report as clearly and concisely as possible in the allotted time. Consequently I had no hesitation in rearranging a sentence or a paragraph here and there to improve its clarity or in adding a sentence or two if some amplification seemed desirable. Likewise I had no hesitation in eliminating any material that was repetitious or irrelevant. There was nothing wrong with this procedure. Every author has the privilege of making changes in the galleys before publication. I would have made far more extensive changes had I not been cautioned to hold all changes to a minimum in order not to disrupt the tight schedule of the printer. In any event it was these corrected galleys that became the official OSS document. A very limited edition was printed, bound, classified "Secret," and distributed to a restricted clientele. It is this document, with some minor corrections that escaped us in 1943, that has been published under the title *The Mind of Adolf Hitler*.

There would be no question of the authenticity of this report as a historical document except for the fact that years later, due to ignorance or carelessness, the Strategic Services Unit, which was assigned the task of evaluating the OSS papers, made the mistake of filing my original first draft manuscript in the National Archives instead of the official OSS document that evolved from it. This is most unfortunate. The National Archives, before the publication of *The Mind of Adolf Hitler*, had sold microfilm or Xerox copies of this manuscript to interested scholars. This has led to some confusion, since the corrections made on the galleys were not made on the manuscript. When one is compared to the other, as Gatzke has done, it is obvious that he will

find that the official document differs in some respects from the manuscript. Since the book is based on the printed document and not on the first draft manuscript the same differences are to be found. Mr. Gatzke, in his review, has listed a number of these divergences, and it is on the basis of these that he would disqualify *The Mind of Adolf Hitler* not only as a "historical document" but also as a faithful reproduction of "the secret psychological report written in 1943." This, it seems to me, is a very strange view for a professional historian to hold in the face of reliable evidence to the contrary. According to this view anything filed in the National Archives, even though it is only a first draft of a subsequent document, automatically acquires a sacrosanct quality as a "historical document" irrespective of what use was made of it later on; any divergence from its text must, *ergo*, be a fraud and consequently unfit for historical consideration. In the case under discussion neither the changes made before publication in 1943 nor those made before publication in 1972 have altered the substance or modified the conclusions of the original report in any way. The report, as published, is, in my opinion, a valid reproduction of a historical document.

WALTER C. LANGER  
Sarasota, Florida

#### TO THE EDITOR:

Permit me the privilege of responding to Hans Gatzke's criticism of my work in his review article.

Let us first come to grips with the Führer's singed scrotum and its missing testicle. Mr. Gatzke says that I have misused the Soviet autopsy report in a "desire to prove [my] case." I was not trying to "prove" anything with the scrotum except monorchism. I also suggested, very tentatively, that the missing part might "conceivably" be the result of self-inflicted castration. We know that Hitler's body was burned in petrol for an indeterminate number of hours. We do not know with any assurance the exact amount of damage done to the scrotum. The Soviet pathologists who report the missing testis note that the scrotal sac was "singed." No photograph of the area was given; the Russian word for singeing or

burning is not used in the published report. If it had been *obozzhena* [partially burned] or if singeing had been extensive, it occurred to me that there might possibly have been enough damage to cover up the scar of self-inflicted mutilation. Not likely, perhaps, but possible. Given Hitler's sadomasochism and his advanced psychopathology, I said it was "conceivable" that self-castration might be one way to account for the manifestly missing testicle. As I noted in my footnote, my consulting psychiatrist thought such mutilation unlikely. But with Hitler strange things were possible; I let the conjecture, as conjecture, stand. Nevertheless Gatzke is probably right: I may have overstated the amount of burning of the scrotum.

Mr. Gatzke suggests that the way a historian cites his evidence may be influenced "unconsciously" by the point he is trying to make. He may be right. Certainly he himself provides us with a persuasive illustration of his own thesis. He strains very hard to discredit Langer's interpretation of an episode in *Mein Kampf* in which an anonymous little boy experienced a primal scene trauma. Langer believes the incident to be autobiographical. Mr. Gatzke quotes from the German edition of Hitler's memoirs to argue that the passage is not autobiographical and does not depict a sexual scene. In quoting from the original, however, Professor Gatzke deletes two rather interesting phrases. In one, Hitler says that such scenes from early childhood are particularly vivid in the memory of gifted people ["bei Begabten"] —a phrase that suggests he had himself in mind. Hitler then describes crowded living conditions, not unlike those of his own family, and writes that this anonymous little boy remembers how members of the family "do not so much live with one another, rather they *press down upon each other* [sondern drücken aufeinander]" (*Mein Kampf*, unabridged edition [Munich, 1941], 32 [italics mine]). Not at all decisive words, but they evoke suggestive images. Dr. Langer did not quote these passages even though they would have strengthened his argument. Professor Gatzke is usually a stickler for full and precise quotation. Did he delete these particular phrases (quite unconsciously) because they did not help his case?

Professor Gatzke has said I am guilty of misusing another source when I concluded that Hitler found it important to associate himself directly and personally with the Nuremberg Racial Laws of 1935, and that he had his own grandmother's experience in mind when he personally set forth as one of the three laws that Aryan women were forbidden to work as housemaids in Jewish homes. Notice the wording in the memoir I cited written by the man who was the legal expert on "racial law" in Hitler's ministry of interior: "*Hitler selbst wollte das Gesetz (das sogenannte Blutschutzgesetz) unterzeichnen. . . . An den strikten Befehlen Hitlers über die drei erwähnten Punkte war nichts zu ändern*" (Bernhard Lösenner, "Das Reichministerium des Innern und die Judengesetzgebung," *Vierteljahrshefte für Zeitgeschichte*, 9 [1961]: 273). These words conveyed to me an extraordinary degree of personal concern with these peculiar strictures. I thought it not excessive to say that Hitler took special pains to dictate the precise language of the racial laws.

With regard to my assertion that Hitler saved his blood and looked at it apprehensively, Mr. Gatzke is correct in saying that the way he looked at his own blood is not given in the source. But Gatzke's concentration on an adverb obscures a main point: according to one of Hitler's secretaries, a generally reliable witness, Hitler had the habit of having his blood drawn out; he saved it and offered to feed it to his secretaries in the form of blood sausage. This kind of testimony with its psychological implications does not, apparently, interest Mr. Gatzke. It does me. But that is another and longer story. Given Hitler's anxiety about the lineage of his father and his preoccupation with the purity of blood, it seems to me not unlikely that he looked at his own specimen "apprehensively."

Mr. Gatzke says he can supply other examples of mistakes and misinterpretations in my early essays. I have written him urging him to tell me about my errors—as I hope all readers will—so I can make corrections before my biography goes to press.

Mr. Gatzke makes a very serious charge with regard to Walter Langer's book, *The Mind of Adolf Hitler: The Secret Wartime Report*. He suggests that both Professor Wil-

liam Langer and I have endorsed a book that is not in fact the OSS report of 1943; he lists discrepancies between the book and a typescript version of the report now available in the National Archives; he writes that the subtitle of the book is "unfounded"; he takes me to task for calling the book a historical document (p. 395).

Before I undertook to write the afterword, I was assured by the publishers that this was indeed the original OSS report, with only the usual minor editorial changes such as the correct spelling of proper names and the ironing out of grammar. After publication, when I pointed out to the publishers discrepancies such as those Mr. Gatzke has noted in his article (p. 395 n. 8), I was informed that the original OSS report, "A Psychological Analysis of Adolph [sic] Hitler," appeared in 1943 in two different versions: there was a first draft in typescript to which Mr. Gatzke refers; but there was also a slightly revised, limited, and classified edition of the report printed especially for the OSS. It is this official printed version that Basic Books has published. (I have subsequently learned that the actual copy used by the publishers was Professor Langer's own personal copy of the printed report which he had received in 1943 by virtue of the fact that he was then chief of the Research and Analysis Branch of the OSS.) When one compares the present book with the printed OSS report of 1943, the discrepancies so carefully listed by Mr. Gatzke do not exist. The publishers had no knowledge of a typescript copy of the report; they believed they were publishing the only extant version. Consequently they could not inform the readers which of the two reports of 1943 they were publishing. Hence Mr. Gatzke's misconception, though deplorable, is perhaps understandable. But before writing his review could he not have telephoned his mentor of thirty years and seminar professor, William Langer, to ascertain the facts of this matter?

As noted in my afterword, Walter Langer made mistakes in his report, but as a pioneering study in the difficult but necessary business of applying psychopathology to so pathological a personality, I believe his book to be of great value. It illustrates A. J. P. Taylor's observa-



tion: "Error can often be fruitful; perfection is sterile."

ROBERT G. L. WAITE  
Williams College

TO THE EDITOR:

As an aspiring psychohistorian I should like to respond to Hans Gatzke's review article. Dr. Gatzke maintains at the outset that "with few exceptions, the preoccupation with the dead Führer has thus far not produced anything that cannot already be found in the still two best books about him"—Heiden's *Der Fuehrer* and Bullock's *Hitler: A Study in Tyranny*. Although conceding that Langer's *The Mind of Adolf Hitler* is one of the exceptions, his criticism of the book's overall approach assures one that, in Gatzke's opinion, the Langer study is anything but exceptional. Yet to be content with Bullock's political biography, which timidly characterizes Hitler the man is simply a "megalomaniac," seems to me to employ an egregious suspension of the very disbelief with which Gatzke observes the recent attempts (so to speak) on Hitler's life.

Gatzke is correct in stressing the importance of clinical experience that historians must bring to the study of any historical figure or era. Such a caveat, however, does not recognize the wealth of theoretical material and published case histories which stand at the ready in any psychohistorian's initial arsenal. Certainly Langer, as an analyst, could and did draw upon a great wealth of material even within the short time he professes to have had under OSS timetables. I fail to see, as Gatzke contends, that Langer's and the research committee's findings were "preconceived" in the same manner as Nazi historians' encomiums to their Führer. The difference, regardless of any inadequacies in the amount of historical data on such a contemporaneous figure, of course lies in the theoretical basis on which the respective treatments were anchored. It hardly needs to be pointed out that Freud's writings offer a significantly firmer ground for objective inquiry than the wishful fantasies of National Socialist propagandists.

As for Langer's interpretation itself, I think Gatzke shows an appreciable lack of under-

standing as to how a psychohistorian properly approaches his material. Although he is correct in questioning the formulistic and reductionistic invocation of the primal scene, Gatzke manifests an inability to discern the layered vicissitudes of sexual imagery, which do not have to be pictorially sexual in expression. But beyond this Gatzke insists upon an appreciation of Hitler that operates only on a conscious level: "There is nothing to indicate that the Führer, always most secretive about his early life, *intended* the passage to be autobiographical, nor does the grim picture painted agree with *what we now know about Hitler's far from dismal childhood*" (my italics). This critique would seem to be historically sound, but in fact it leaves the most important question unanswered: why did *Hitler* see things the way he did? Not Bradley Smith. Not Franz Jetzinger. Not August Kubizek. *Mein Kampf* is a personal document, reflective of the man who set it to paper, not a history and analysis of Austro-German family dynamics in the late nineteenth century. This is not to say that Langer's study is definitive; it perforce was overly reductionistic given the paucity of biographical data that is just now surfacing to help refine the image of the man throughout his entire life. But the intensity and scope of Hitler's projections in *Mein Kampf* alone are revealed when he reflects upon the examples of family life quoted by Langer and, in turn, by Gatzke: "I have seen this in hundreds of instances." Rather, he has relived his own perceived experience a fateful multitude of times.

One final observation: Gatzke claims that a perusal of the "Source-Book" included in the original report at the National Archives provides "evidence to support almost any image of the man—from repulsive, dirty, lazy, and sexually perverted psychopath to attractive, neat, hardworking, and sexually normal statesman." One would be hard pressed to describe any more succinctly the neurotic Hitler and the obsessive-compulsive character armor so symptomatic of Central European authoritarian family structure.

GEOFFREY COCKS  
University of California,  
Los Angeles

## PROFESSOR GATZKE REPLIES:

I shall try to keep my comments brief. But since most of the writers are in one way or another connected with *The Mind of Adolf Hitler*, their objections to my review deserve careful consideration.

To begin with, Professor William Langer is astonished "that a professional historical journal should devote so many valuable pages to a book that is not history at all." He has apparently forgotten that in his foreword to his brother's book he himself refers to it as "a milestone marking new and fruitful directions in historical study," and that the dust jacket proclaims it "a masterpiece of 'psycho-historical reconstruction.'" One might argue whether the quality of the book justified devoting so much space to it. But here both the editor of the *AHR* and the reviewer felt that given the publicity the book has received and the debate it has caused, it merited the attention it was given.

Professor Langer then touches on his own relations to his brother's report for the OSS, from which the book derives. I already stated (and expressed regret) in my review that neither William Langer nor his fellow historians in the OSS had anything to do with the report; it would doubtless have been improved if they had. Professor Langer's objection that historians could hardly have contributed to an evaluation of Hitler's psyche misses my point. All I said was that historians could have checked the validity of the sources on which Walter Langer's report was based. Professor Langer states further that the members of his group were too preoccupied with other matters to pay much attention to his brother's work. This may have been so. It should be noted, however, that Dr. Langer did collaborate with his brother and apparently some other historians in the OSS on at least one earlier occasion (see "Memorandum to Dr. William L. Langer from Walter C. Langer. Subject: Analysis of Hitler's Speech of April 26, 1942," Un-classified Historical OSS Records, National Archives, Washington, D.C.).

The bulk of Professor Langer's letter deals with what he calls "one of the strangest features" of my review—my claim "that the book is not what it purports to be." Here, briefly, are the facts:

When Walter Langer's book was published, I already had a copy of the original manuscript from the National Archives. Comparing the two, I noted a number of discrepancies. I wrote to the publisher asking for an explanation. It took almost five months to get the whole story straight.

It seems that the OSS, before circulating copies of Langer's report in 1943, had prepared a printed version. (This apparently was not known to the National Archives or even to some of the people connected with Langer's book.) The publishers at first claimed that the OSS, in doing so, had only made some minor copyediting changes. That claim proved false. After continued probing I discovered that not only the OSS but also Dr. Langer himself had made considerable changes, the latter adding long passages and whole paragraphs. This was the version that Basic Books decided to publish. Before doing so, however, they did their own copyediting, and, in addition, Dr. Langer made a second round of changes. These, I suppose, are the ones Professor Langer refers to as "piddling." Maybe so. Though I wonder if the change in terminology made just before publication—from "hysteric" to "neurotic psychopath"—in the basic diagnosis of Hitler's mental condition was really so unimportant.

So much for how the Langer book came about. In his introduction Walter Langer tells a quite different story. After recounting the last-minute rush to get his manuscript done in 1943, he concludes that because of that rush "the first draft automatically became the one and only draft. In this way the study was completed at the appointed hour." Dr. Langer then goes on to regret not to have been able to revise his draft since such revision and discussion with his collaborators "would have resulted in a much better product."

Here, then, are two different versions of the book's origins. In commenting on this issue in my review, I said: "Recent correspondence with the publishers has revealed that the original manuscript was changed and edited several times by Dr. Langer and others, both in 1943 and again before publication. The claim made on the dust jacket, therefore, that 'here is the secret psychological report written in 1943,' and the statement by Walter Langer in his introduction and by Robert G. L. Waite in his

afterword that the book presents a 'historical document,' are hardly justified." Nevertheless, Professor Langer accuses me of imputing intentional misrepresentation and fraud to those connected with the publication of his brother's book. I do not think the evidence warrants such an accusation.

The letter by Walter Langer does not add much to his brother's charges against my "allegations and insinuations" concerning the origins of his book. Dr. Langer's statement that he merely added "a sentence or two" before his report was printed in 1943 understandably minimizes the changes he made at that time; and the alterations made immediately before publication in 1972 are merely mentioned in passing but not explained. I agree with Dr. Langer that every author has the privilege of making such changes, but only if he did not expressly tell his readers that he had *not* done so, as Dr. Langer did in his introduction (see above).

The main interest of Walter Langer's letter, however, is in what he says about his relations with Dr. Henry A. Murray, whose analysis of Hitler was prepared at the same time as his own and in some parts bears considerable resemblance to *The Mind of Adolf Hitler*. First of all, he takes me to task for not realizing that the unnamed collaborator who, according to Langer's introduction, defected from his project was none other than Dr. Murray. How is one to know thirty years later that Dr. Murray (whose name appears nowhere in the book) "was at Harvard at that time and the other two collaborators [also unnamed] lived in New York," and that these domestic arrangements made it "rather obvious who the defector was." I see nothing obvious about all this. Having failed to guess who the anonymous culprit was, I am then berated for not realizing that the statement "unfortunately, not a word was ever received from him" refers to Dr. Murray. Instead of giving his manuscript to Langer, we are told, Murray diverted it "into an unrelated channel."

The Murray manuscript, as explained in my review, is in the Franklin D. Roosevelt Library at Hyde Park, New York. With it is a statement by Henry Field, dated October 29, 1943, explaining that "the cost of typing, stencilling

and mimeographing [of this manuscript] was paid by O.S.S." This, of course, does not necessarily mean that the OSS showed Murray's report to Dr. Langer. Still the similarity between Langer's and Murray's reports is striking. For example, Langer (*The Mind of Adolf Hitler*, p. 209) writes: "Hitler may get killed in battle. . . . This would be most undesirable from our point of view because his death would serve as an example to his followers to fight on with fanatical, death-defying determination to the bitter end . . . and insure his immortality." And Murray ("Analysis of the Personality of Adolph [*sic*] Hitler," pp. 30-31) writes: "Hitler may get himself killed leading his élite troops in battle . . . which would be very undesirable from our point of view, first because his death would serve as an example to all his followers to fight with fanatical death-defying energy to the bitter end, and second, because it would insure Hitler's immortality." Objective investigators may indeed agree with Dr. Langer "that there must be more to this psychoanalytic approach than meets the eye."

The issue of whether the published version of Walter Langer's book was the original one is also dealt with in the letter by Professor Waite. Since I have already explained my position, I need not repeat it. As for my criticism of Professor Waite's own work, I can be brief. On the specific points he mentions, which are not all the ones I raised in my review, nothing he says makes me want to change my earlier position. Just two observations: (1) The statement in the Russian autopsy report mentioned by Mr. Waite does not merely say that Hitler's genital area was "singled," but, as I already pointed out in my review, that it was "singled but preserved." Mr. Waite's disquisition, therefore, on whether "singled" meant partial or extensive burning seems somewhat labored. (2) The source cited by Professor Waite in one of his articles for the statement that Hitler's doctor drew blood from him "and preserved it in test tubes, so that Hitler could gaze at it apprehensively," actually states only that the doctor performed the bleeding operation—the test tubes and the apprehensive gazing are Waite's invention. I think, therefore, that it is Mr. Waite's "concentration on an adverb" that obscures my main point, namely,

that Mr. Waite, in citing evidence, sometimes gives rather free rein to his imagination. I might add, as Mr. Waite says in his letter, that I am preparing a list of additional mistakes or misinterpretations that I have found in his work. As he suggests, I shall be glad to hear directly from any reader interested in it.

Besides taking issue with my criticism of his own work Professor Waite charges that in my discussion of the primal scene incident I deleted some phrases because they did not help my case. Here, I think, he himself is straining a bit hard. I was aware, of course, of the passages he cites, as, I am sure, Dr. Langer was. But since the latter did not consider them worth mentioning I saw no reason to attribute to them the significance they seem to hold for Mr. Waite. All I did was take the passages quoted by Dr. Langer and retranslate them. There was no reason why I should add further passages from *Mein Kampf* which, in any event, would not have changed my interpretation of the evidence for the alleged primal scene.

The letter by Geoffrey Cocks comes close to what I had in mind when wishing, in my review, that Langer's book would lead to fruitful debate among historians and psychoanalysts. We obviously disagree on some points—I think Bullock's is a better book than he does, and he thinks I do not really understand what psychohistorians are all about. I am not sure about the latter. I realize that my evaluation of the primal scene evidence is rather literal or unimaginative. But the reason is not so much my "inability to discern the vicissitudes of sexual imagery" as my belief that the evidence for so crucial an event in an analysis of Hitler should for that very reason be scrutinized with special care. In addition to what I have said in my review, I am not certain how personal a document *Mein Kampf* really is, and I would more readily accept evidence from a source like Hitler's "Table Talk" (which, of course, was not as yet available to Langer). Should corroborating evidence for the primal scene turn up, I would be the first to welcome it as a major aid to psychohistorians in their efforts to understand Hitler.

HANS W. GATZKE  
Yale University

*The following exchange of letters has been received in connection with the publication of Stephen H. Haliczer's article, "The Castilian Urban Patriate and the Jewish Expulsions of 1480-92," AHR, 78 (1973): 35-58.*

TO THE EDITOR:

It is good to have more sense made of the expulsion of the Jews from Spain. Stephen H. Haliczer's article, however, raised more questions for me than it answered. He established that *conversos* were influential in the decree of expulsion, yet also discussed *conversos* as managing to escape the Inquisition. If we accept, as Haliczer apparently does, that *conversos* called in by the Inquisition were Judaizers, must we then conclude that secret Jews desired the expulsion of their religious fellows? Or was it only the true New Christians who wanted to separate Jews from Spanish society? He has indicated that both sorts of *conversos* sat on town councils, but not whether both shared the *converso* attitude toward the Jews with which he is concerned. In other words, to say that *conversos* were so influential that they escaped the Inquisition, served on town councils, and influenced the decree expelling the Jews is not enough. We must know which sort, true or feigned, of *conversos* did what. Otherwise, many of his supporting arguments make little sense, for vital links are left wide open. The idea that *conversos* were active in anti-Jewish movements is not new. (For example, see Henry Kamen, *The Spanish Inquisition* [1965], 38-40.) Haliczer's connection of *conversos* to town oligarchies accords with Ruth Pike's work on Seville, most recently, and is interesting, but only as a step toward explanation. We still need more information on the frames of mind, as well as on the activities, of *conversos* in order to understand them and their place in the history of Spain and Spanish America.

PEGGY K. LISS  
Akron, Ohio

TO THE EDITOR:

A central thesis of Stephen Haliczer's article on the expulsion of the Jews from Spain is that

the prime movers behind that expulsion were the *conversos*, who applied pressure on the crown through their influence on the town councils.

The crucial steps in establishing this thesis are the following: (1) *conversos* were sufficiently influential in the town councils to frustrate certain policies of the Inquisition (pp. 43-47); (2) it was "the *converso* position" that Jews should be prevented from contact with Christians and be made to "embrace Christianity, using force if necessary" (pp. 47-49); (3) town councils are known to have segregated Jews shortly before the expulsion. Since this was *converso* policy, and since *conversos* had influence in the town councils, it follows that this policy of the councils and hence the expulsion itself resulted from *converso* pressure.

This argument is far from conclusive even if the premises are granted, but its greatest weakness lies in the assertion that expelling the Jews was, in fact, "the *converso* position." Since an expulsion of the Jews would clearly add to the number of Judaizing *conversos* and thus to increased inquisitorial pressure, the wisdom of such a *converso* policy, at least for the short term, would be dubious at best. Moreover, the author's evidence that this stand was that of the "*converso* intellectuals" consists of precisely one quotation from a *converso* priest in a book written in 1465 (pp. 48-49). Two lines after this citation, Dr. Haliczzer is already referring to this view as "the *converso* position" (p. 49). A few earlier quotations showing that *conversos* insisted on the unity of Old and New Christians do not, in themselves, constitute even the slightest evidence that New Christians wanted the Jews expelled, and even Dr. Haliczzer notes that the one quotation from Alonso de Oropesa is "crucial" to his case.

There is, furthermore, another serious methodological flaw in the article. If, in fact, the moving force behind the expulsion was *converso* pressure, contemporary Jews would obviously have known this and resented it deeply. The article, nevertheless, contains no indication that Dr. Haliczzer consulted the considerable Jewish literature of the period to determine whether or not Jews ever accused New Christians of responsibility for the expulsion. This would not be a very difficult

task, because there exists an English summary of most of the pejorative Jewish comments about the *conversos* (B. Netanyahu, *The Marranos of Spain from the Late XIVth to the Early XVIth Century According to Contemporary Hebrew Sources*, 1966). Netanyahu's thesis that most *conversos* were good Christians is itself controversial (see G. D. Cohen's review in *Jewish Social Studies*, 29 [1967]: 178-84; and Y. H. Yerushalmi, *From Spanish Court to Italian Ghetto* [1971], 21-42), but it is revealing that his exhaustive study (especially pp. 135-208) reflects no explicit charge that *conversos* brought about the expulsion of the Jews. Perhaps Dr. Haliczzer can find some explanation for this, or perhaps he will argue (implausibly, in my opinion) that such a charge is implicit in some of the stronger anti-*converso* statements that can be found (see Netanyahu, pp. 175, 180-81), but failure to consider contemporary Jewish sources at all constitutes an extremely significant oversight.

DAVID BERGER  
Brooklyn College,  
City University of New York

#### TO THE EDITOR:

Stephen H. Haliczzer's excellent article traces the edict of expulsion to the alliance between the crown and the urban oligarchies of the Castilian cities. According to Professor Haliczzer, the Castilian *conversos* enjoyed a prominent social, economic, and political status in Castile. Although they achieved this status after the forced mass conversions following the anti-Jewish riots of 1391, the *conversos* remained vulnerable to the growing enmity of the Old Christian element within the urban oligarchies. To protect their newly gained status, the *conversos* adopted an anti-Semitic program. This program called for segregation of the Jews, their exclusion from economic life, and eventually their expulsion from the land.

I found Professor Haliczzer's article extremely interesting, but I believe he has neglected the existing anti-Semitic program of the urban oligarchies before 1391 and the influx of *conversos* into city government. An examination of the *Libro de las Cortes* (edited by the Real Academia de la Historia [Madrid, 1861,



1863]) will show the long-standing anti-Semitic attitude of the Castilian urban oligarchies.

Beginning as early as 1252 at the Cortes of Seville, the urban representatives protested against the power and influence of the Jews. They asked for, and the Cortes repeatedly granted throughout the next century and a half, stringent anti-Jewish legislation. The edict of expulsion of 1492 was the end product of a long historical process. The anti-Jewish program began earlier than 1391 when *conversos* were not yet incorporated into the urban oligarchies. The ordinances of the Cortes show that segregation of Jews and attempts to exclude them from the economic and political life of Castile were well under way before the mass conversions of 1391. Let us review some of these measures.

At the Cortes of Valladolid (1258), Jews were forbidden to wear white clothes or gold ornaments. They were ordered to dress in black or dark-colored clothes unless personally exempted by the king. At the *Ayuntamiento* of Jerez (1268) these controls became more strict and included an extensive list of banned feminine garments plus penalties for those disobeying the law. In 1313 the Cortes of Palencia ordered the Jews to wear a yellow circle in the front and back of their upper garments as a sign of their religion. The Cortes of Toro (1371) confirmed this regulation. Ordinances controlling the type and style of the clothes worn by Jews were not limited to the Cortes already mentioned.

As early as the Cortes of 1268 and in most that followed, measures were enacted forbidding Jews to bear Christian names and to marry or cohabit with Christians. Jews could not be used as nannies for Christian babies. Ordinances restricting Jews to specific parts of the cities were passed at the Cortes of Valladolid (1322) as well as later Cortes.

The urban procurators did not limit their petitions to the social segregation of the Jews. Their petitions, born of the economic crisis of the mid-thirteenth century, reflected the desire of the urban oligarchies to destroy the economic and political position of the Jews. At the Cortes of Palencia (1286) and most Cortes for the following fifty-three years, the *caballeros villanos* and good men claimed the

exclusive right to collect taxes while protesting against tax farming (done mainly by Jews).

Legislation limiting the interest rate of Jewish usury can be found in the ordinances of the Cortes of Valladolid (1258) and in almost every Cortes thereafter. The urban procurators also asked the king for partial or total cancellation of their debts to Jewish moneylenders. They complained against the Jews' acquisition of land and real estate under municipal jurisdiction and opposed Jewish ownership of land, as well as their exemption from certain taxes.

Politically, the *concejos* sought to exclude Jews from financial and administrative positions in royal government. Above all, the *concejos* fought to end Jewish autonomy within the cities and to bring them under their jurisdiction. The urban oligarchies demanded that Jewish affairs within the cities be dealt with by municipal officials and not by special *alcaldes* assigned to the Jewish *aljamas*.

Regardless of the anti-Semitic program of the urban oligarchies from 1252 on, the Jews counted on royal support throughout the period. Their favored position reached its peak under Pedro I (1350-69) and Enrique II (1369-79). Their growing influence continued to draw bitter protests from the urban oligarchies. At the Cortes of Toro (1371), the second proviso of the ordinances summarized in angry words one hundred and twenty years of anti-Semitic programs. The language clearly indicated that the municipalities could do without the Jews altogether. If the Jews remained in the kingdom, it was because the king wished it. But if the Jews were to remain in the realm, the urban oligarchies asked that they be socially and politically segregated and economically excluded in the manner I have indicated above.

Thus anti-Semitic sentiment appeared among the urban oligarchies long before 1492 or 1391. A program of segregation, economic exclusion, and even expulsion did not originate with *converso* intellectuals, nor did their program represent a departure from the traditional attitude of the urban oligarchies in Castile.

TEOFILO F. RUIZ  
Brooklyn College,  
City University of New York

## PROFESSOR HALICZER REPLIES:

I should like to begin my reply by thanking Professors Liss, Berger, and Ruiz for their interesting and provocative letters. The Jewish expulsion was a very complicated affair, and I certainly do not think that I have fully covered all of its many aspects in my article.

Although I agree with Professor Liss's suggestion that it would be useful to have more information on the *conversos*' "frame of mind" I feel that the real or feigned conversion of a *converso* city councilor would have played only a small part in his decision to support the anti-Semitic ordinances that led to expulsion. After all, both real and feigned *conversos* were threatened by the Inquisition and both sat on city councils that collectively viewed the existence of separate Jewish communities as an intolerable intrusion into their control of local politics. In short, the political motivations for supporting expulsion united the diverse elements on the councils and cut across the real convert, feigned convert boundary.

The two major objections raised by Professor Berger are that expulsion would have resulted in more Judaizers and increased inquisitorial pressure and that contemporary Jews would "obviously" have known it if *conversos* were the moving force behind the expulsion. To the first point I would reply that the removal of the Jews would have undoubtedly fostered the *conversos*' more rapid integration into the wider Christian community (the major goal of *converso* leaders since mid-century), and that anything that helped to accomplish this would in fact lessen Old Christian hostility and inquisitorial pressure.

With regard to the second argument I really doubt that Jews would have had any way of becoming aware of the nature of the *conversos*' anti-Semitic activities on city councils and in Hermandad juntas since the former were held in secret session and the Jews had no representatives in the latter. Under these conditions it would have been nearly impossible for Jews to know which elements on these bodies worked against them with the greatest persistency. I believe that Professor Netanyahu's book supports my general argument by demonstrating the bitter hostility of Jews and converts in the years before the expulsion.

I should like especially to thank Professor Ruiz for pointing out the many early instances of anti-Semitic feeling expressed at the level of the Cortes by the representatives of the Castilian urban oligarchy. In doing so, he supports my attempt to reorient the debate about the expulsion and look at it as a royal response to overwhelming political pressure from the periphery rather than a genuine royal policy aimed at achieving political homogeneity.

I tend to think, however, that Professor Ruiz has missed some essential points. As Américo Castro and others have pointed out, the anti-Semitic ordinances passed at the Castilian Cortes before 1391 were seldom enforced. What is really unique about the period 1480-92 is the rapid enforcement on a municipal level of anti-Semitic laws passed at the Cortes of Toledo and the passage of local ordinances that implemented earlier anti-Semitic legislation. I suggest that this change in enforcement was brought about in part because of the influx of *conversos* into the ranks of the Castilian urban elite during the fifteenth century.

STEPHEN HALICZER  
Northern Illinois University

## TO THE EDITOR:

Being in complete agreement with Gabriel Jackson's interpretation of the late Américo Castro's recent and earlier books on the Spaniards, I am reluctant to point out a merely technical error in his review (*AHR*, 78 [1973]: 114-16). But it is incorrect to refer to Cardinal (Pedro González de) Mendoza as a *converso*. The "Grand Cardinal" of Spain was a son of the marquis of Santillana, descendant of an ancient Basque-Castilian lineage without any known Jewish antecedents. Though the famous *Tizón de la Nobleza*—a "document" of dubious authenticity, perhaps mostly fabrication, that attributes the "taint" of Jewish or Moorish ancestry to the entire Spanish higher nobility (reprinted in Julio Caro Baroja, *Los judíos en la España moderna y contemporánea* [Madrid, 1961], 3: 287-99)—includes many Mendozas, these belong to the third-following generation. Even if the cardinal's great-grand-

children and grandnephews possibly had some non-Christian admixture from eight or ten generations back, none of the aristocrats, whether "tainted" or not, was called *converso*. Strictly speaking, this term should apply only to a former Jew like the Rabbi Solomon Ha-Levi who, after his baptism, took the name of Pablo de Santa María; but eventually it came to comprise the descendants (born and raised as Christians) of such converted Jews as well, the "caste" consisting of clerics, merchants, and professionals—in short, a large part of the middle classes. Of this class was Hernando del Pulgar, a secretary of the Catholic Kings, who wrote the letter to Cardinal Mendoza cited by Professor Jackson. Incidentally, Castro did not have to demonstrate Pulgar's status as *converso* from that letter. Pulgar had already been listed as one of the neophytes by, among others, Juan de Mariana and José Amador de los Ríos.

ERIKA SPIVAKOVSKY  
Westport, Connecticut

#### TO THE EDITOR:

I find it necessary to present certain crucial aspects of my book, *Business and Politics in America from the Age of Jackson to the Civil War: A Career Biography of W. W. Corcoran*, with which Robert Sharkey had difficulty in his review (*AHR*, 78 [1973]: 163-64). Not that I am unappreciative of his efforts; the book offers an extraordinary amount of new and important material very compactly, much of it in fields in which historians have not felt at ease. Nevertheless, I believe Sharkey's review illustrates a widespread problem of operating assumptions and approaches, and especially an inability to grasp the general conclusions toward which the massive factual presentation points implicitly and explicitly.

The book's basic theme (inductively derived) is that American political and economic life in the period discussed was pervaded by the dual problems of a vast increase in the scale of power, and the multiplication of centers of power, with resulting gross political and economic instability, separately and in interaction. These conclusions emerged from a concrete and heavily documented examination of the structure and development of banking and

railroads (filling in great historiographical gaps in the process). I demonstrated some of the consequences not only on the national but also on the state level in various parts of the country.

I confess I am astonished that Sharkey barely mentioned the existence, and did not convey the substance, of this thesis, which is clearly set forth in and dominates the preface, the concluding "Summation" to part 1, and the concluding chapter of the book, as well as being suggested in transitional passages and clearly implicit throughout.

The review gave the personal "corruption" an emphasis that became excessive in proportion to the neglect of the major theme. I would not belittle individual responsibility—far from it, as I will show. The Corcorans must bear their share; but it is too simple to portray them as monsters of corruption, nor would their failings bear comparison with, say, the experience of Mexico. (Is it a symptom of enduring parochialism that there was no mention of the extraordinary chapter on the Mexican-British-American triangle?) Rather, the emphasis of the book is on the civilization of which Corcoran was a representative part, and to the exigencies of which he responded, as well as on his individual role. In shifting the primary emphasis to personal corruption, the review verges upon making him a scapegoat for the institutional structures and the drives of the society as a whole.

The personalism comes through in other ways. There seems to be, for example, a kind of escapism in the craving for information, beyond what is available and historically significant, about Corcoran "as a man"; or is it only a yearning for simplicity? But let us look at a more consequential example. Sharkey could not comprehend the chapter on railroads and politics: "One is at a loss to understand what it has to do with Corcoran." The land speculation associated Corcoran with twenty-six representatives of major factions, chief of them Stephen A. Douglas, some of whom in direct consequence became advocates of specific projects, which in turn directly contributed to the turmoil of the Kansas-Nebraska conflict in nation, state, and territory. At what point is the historian supposed to stop pursuing the direct chain of conse-

quences, in the origins of which his subject participates?

Nor is this only a philosophical problem; it bears heavily upon the writing—and the living—of history. I say with dismay that there are few historians who would have made the discoveries I made about the structure of American banking, precisely because they would not have followed the direct chain of consequences to its ultimate ramifications. Similarly the problem of the railroad origins of Kansas-Nebraska, first posed by F. H. Hodder six decades ago, would still remain mysterious if some other biographer, forgetting his role as historian, had ceased his quest at the point at which Corcoran became “only” one of many, rather than personally a major causal figure. (It may be apropos to note that the basic sources for these discoveries have been available to historians at least since the 1920s.)

The perceptive reader will note that I explicitly studied the consequences of Corcoran’s acts, not only when he was one of the “big” participants, but also when he was a representative participant. To neglect his significance as a representative participant would have meant writing history according to a version of the “great man” formula; it would have meant sacrificing history to biography, instead of achieving some synthesis of the two modes of inquiry.

HENRY COHEN  
*Loyola University,  
Chicago*

TO THE EDITOR:

In his review of *The Foundations of the Modern World* (*AHR*, 77 [1973]: 1406–07) by Louis Gottschalk and others, Franklin L. Ford quotes Professor Roland Mousnier’s printed criticism of that volume as if it were only “an encyclopedic work of random reference.” From among the comments of Professor Mousnier and his colleagues only those were printed that seemed to the Secretariat of the International Commission as relevant to substantial weaknesses of the manuscript. Hence the following final paragraph of Professor Mousnier’s remarks was omitted: “But, after having made our observations and expressed our reservations in all sincerity, as the International

Commission had asked us to do, it is with the same sincerity, taking into account the difficulty of the task confronted by M. Louis Gottschalk and his collaborators and fully aware of the diversity of the possible solutions, each with its advantages and disadvantages, that I thank and congratulate, in my name and in the name of my collaborators, the authors of this book for their objectivity, their idealism, their love of humanity, and for their rich harvest of facts which are going to enlighten all men in their striving toward the happiness and the perfection of the human species.”

My aim in citing Professor Mousnier’s comment is that his more complete critique may be known.

LOUIS GOTTSCHALK  
*University of Chicago*

TO THE EDITOR:

Permit me to add a few remarks to the discussion of Gerhard Masur’s *Imperial Berlin* (*AHR*, 78 [1973]: 191–92). Critic and author have spoken. Here is what a historian who himself has grown up in “Imperial Berlin,” prior to 1914, jotted down when he first read the book: this book has an all-pervading “theme”—the coexistence of the Prussian tradition that had been enlarged to become “Imperial,” and of the “World-City,” industrial, exuberant, and critical. It is the great achievement of the book that the “theme” is never presented as a thesis, but rather in the form of concrete description. Only by reading this book have I become fully aware of the double character of the city of my youth. Perhaps this reaction may be of interest to your readers.

DIETRICH GERHARD  
*Washington University*

TO THE EDITOR:

I regret the printer’s error that represents J. H. Hexter’s characterization of the historian’s mode of discourse as “possessive” in my review of his *The History Primer* and *Doing History* in the April 1973 *AHR* (pp. 402–03). The word I quoted in my manuscript was “processive.”

W. WARREN WAGAR  
*State University of New York,  
Binghamton*

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## Recent Deaths

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HARRY J. BENDA, professor of history and associate chairman of Southeast Asia Studies at Yale died on October 26, 1971; two days later he would have been fifty-two.

A heart attack terminated the life of the nation's leading expert on Indonesian and Southeast Asian history and ended a career that had only begun in the 1950s. Benda was born a few months after the Versailles peace treaty in newly created Czechoslovakia (Liberec) as a son of Jewish parents. In 1938, under the threat of Nazi occupation, he left his country and found asylum in the Dutch East Indies where he entered a promising business career. In 1943 the Japanese occupation army (following a directive of their Axis partner in Berlin?) imprisoned Jewish emigrants in areas under their control, and it was in the internment camp that Benda decided to pursue an academic career. Conversations with fellow prisoners, in particular with the eminent Dutch scholar W. F. Wertheim, led him to reflect on what he was witnessing—the impact of the Japanese in a colonial setting.

When, after a time of intense studies in New Zealand (1946–52), Benda was accepted in the graduate school of Cornell University, he wrote his dissertation on the emergence of Indonesian Islam in the war period as a political potential. He thoroughly analyzed Dutch policy vis-à-vis Islam in the colonial period and compared it with the different approach of the Japanese, who needed a reliable ally in their fight against the West. By their enforcement of a united front of the various Islamic organizations, which the Dutch had always tried to prevent, the Japanese unleashed new dynamics that were deeply to influence postwar developments in Indonesia.

This work, published under the title *The Crescent and the Rising Sun* (1958), earned him, after a first appointment at the University of Rochester (1955–59), an associate professorship in the department of history at Yale (1959); his book is still a classic in the field of Indonesian studies.

Benda's way to Yale, where he became professor of history in 1966, thus contrasts sharply with that of his "predecessor" Clive Day (1871–1951), perhaps the first American scholar ever to publish a scholarly work on Indonesia (*The Policy and Administration of the Dutch in Java*, 1904). Day was as wholly a Yale man as one well could be: being a descendant of a former president of Yale, he himself received his B.A. (1892) and his Ph.D. (1899) from Yale and was on its faculty in the department of history from 1898 to 1936, when he retired. Although Day did not continue his studies on Indonesia (his interest lay with the history of commerce), he left to his "successor" an invaluable collection of material on colonial Indonesia, which was gratefully used by the latter and the steadily growing number of his students.

Benda's next publication, together with Ruth McVey, was a translation and annotation of Dutch documents, dealing with *The Communist Uprisings of 1926–1927 in Indonesia* (1960). Here it becomes quite apparent, what was already discernible in his dissertation, that his scholarly concern was with the colonial period rather than with contemporary events. He was fascinated with the phenomenon of "indigenous response" to "Western input" and he soon went beyond Indonesia and examined Southeast Asian history with this question in mind. For some time he was struggling



with the role of elites in transitional societies, and he wrote stimulating articles on their function in the Western and the non-Western world that are now republished in a nice memorial volume (*Continuity and Change in Southeast Asia. Collected Journal Articles of H. J. Benda*, 1972). He soon discovered, however, that elitism would not provide the answer for his questions, in particular not the modern Westernized intelligentsia. He repeatedly remarked "that Western impact had not necessarily affected the fundamental character of Southeast Asia itself." So he paid increasing attention to prevailing pre-Western values and orientations, to the world of the peasants and their beliefs, to the "Indianized," "Sinicized," and "Islamized" traditions in their local context. These themes were colorfully depicted in his selected readings *The World of Southeast Asia* (coauthored with J. A. Larkin, 1967) and perceptively analyzed in *A History of Modern Southeast Asia* (coauthored with J. Bastin, 1968), his last monograph, and in various articles now easily accessible in the above mentioned volume.

No wonder that the man who was pleading for a better understanding of the peoples of Southeast Asia in scholarly approaches was a severe critic of the American involvement in that part of the world and that he was bitter when he saw how little his advice (he lectured regularly at the Foreign Service Institute in Washington) was heard. In his personal relations Benda was the most amiable man. He was a loving husband to Eva, who had survived the terror of Auschwitz, and an affectionate father to Peter and Susan. The number of his personal friends was seemingly unlimited—at national and international conferences he was always a center of attraction. A highlight in his short but intense and fruitful career was the invitation by the government in Singapore to found and be the first director of the local Institute of Southeast Asian Studies (1968–69), which aptly reflects the international recognition of Benda and his work.

BERNHARD DAHM  
University of Kiel  
Yale University

JOHN DANIEL BRIGHT, professor emeritus of history at Washburn University, died February

9, 1973. He was seventy-five years old. Dr. Bright received his A.B. from Manchester College and his M.A. and Ph.D. from the University of Wisconsin. From 1926 to 1939 he was chairman of the department of history at McPherson College. Joining the faculty of Washburn University in 1939, he served as a professor of history and chairman of the department of American citizenship until 1962. Dr. Bright was a life member of the AHA.

RUSSELL C. EWING, professor of history at the University of Arizona and head of the department from 1958 to 1969, died suddenly at his home in Tucson on October 14, 1972, at the age of sixty-six.

Born in Manhattan, Kansas, he moved with his family to San Francisco at the age of ten. He received his B.A. degree in political science in 1929 from the University of California, Berkeley, where, as a track star and All-American halfback, he excelled in athletics as well as scholarship. Entering graduate school in the fall, he soon came under the stimulating guidance of Herbert Eugene Bolton with whom he took his M.A. in 1931 and his Ph.D. in 1934. Academic positions were scarce indeed in the midst of the depression, so like many others he entered government service, accepting a position as regional historian for the National Park Service. Two years later an acceptable opportunity arose to return to his vocation, and he joined the faculty of the University of Arizona. There he found a congenial academic home as a teacher and for the pursuit of his major research interests, the history of Mexico and the Spanish borderlands.

In the years following, Ewing published a number of articles and coauthored or edited several volumes: *Greater America* (1945), *The United States and Latin America* (1960), *Arizona, Its People and Resources* (1960), and *Six Faces of Mexico* (1966). At the time of his death he was writing a history of Mexico and had completed research on a guide to the history of the Spanish borderlands.

He assumed the leadership of the department at a difficult moment in its growth and development. Early in his tenure he guided the separation of political science into a department of government and undertook the inauguration of a doctoral program in history. Throughout

his decade in office, rapidly expanding enrollments necessitated almost constant faculty recruitment and curriculum revision. These challenges he met with efficiency and equanimity.

During his long tenure at Arizona, Ewing's attention to teaching and research was diverted and sometimes refreshed by several memorable episodes. In the summer of 1940 he joined Professor Bolton and several other scholars in retracing much of the 2,500 miles covered by Francisco Vázquez Coronado on his famous expedition in 1540, an accomplishment in which he took a good deal of pride and pleasure. The outbreak of World War II brought experience of a different sort. He volunteered for the United States Navy and was assigned as an instructor in the Naval Officers School at the university. He left the service in 1946 as a lieutenant commander. In 1956-57 Ewing taught as a Mundt-Smith grantee at the University of the Andes, Bogota, Colombia, where he had the unique opportunity to witness the revolution that ended the Rojas Pinilla dictatorship in that country.

Professor Ewing's services to his students, his profession, the university, and the community of Tucson, numerous and time consuming, reflected the high ideals of personal dedication he brought to every task as well as the trust placed in his integrity and judgment by everyone who knew him. All of his endeavors were marked by a compassionate concern for the well-being of others and by a constant effort to judge fairly his fellow men past and present. These fine qualities were well concealed behind a modest mien that endeared him to both his colleagues and his students.

ROBERT PAUL BROWDER  
University of Arizona

HERBERT HEATON was a Yorkshireman. Born on June 6, 1890, at Silsden, Yorkshire, of Fred and Eva Heaton, he was educated at Batley Grammar School and then at Morley Secondary School. A scholarship took him to Leeds University where he won first class history honors in 1911; in the same year he was Rutson Research Scholar at that institution. He also studied at the London School of Economics.

From 1912 to 1914 Herbert Heaton was an assistant lecturer at the University of Birmingham. Then the threat of weak lungs took him

to the Antipodes by way of Suez, India, and Singapore. For the next three years he was lecturer in history and economics at the University of Tasmania. He then moved to the mainland and was lecturer in economics and director of tutorial classes at the University of Adelaide until 1925.

As part of the eastern movement in the British Empire Herbert Heaton journeyed from Australia to Queen's University at Kingston, Canada, where he was Sir John A. Macdonald Professor of Economics and Political Science. In 1927 he joined the history department at the University of Minnesota. There his wit and his wide-ranging knowledge of man's attempts to make a living from ancient days to the present became proverbial. He continued at the University of Minnesota until his retirement, after four years as chairman, in 1958.

Many honors came his way. He received a Master of Arts degree from Leeds University, a Master of Commerce from Birmingham University, and a Doctor of Literature from Leeds. In 1924 he was special lecturer on Australian problems in London, Cambridge, Toronto, and Edmonton. He was a Guggenheim fellow in 1931-32, and in later years he was a visiting professor at Princeton, Johns Hopkins, and the Universities of Utah, British Columbia, Texas, California, Michigan State, and Pennsylvania State. He was the secretary of the Social Science Research Council's committee on research in economic history. From 1948 to 1950 he served as president of the Economic History Association.

Heaton's first publication was *Welfare Work* in 1919. But better known and an earnest of the future was his *Yorkshire Woollen and Worsted Industries* that saw print in 1920 and was reprinted in 1965. His *Modern Economic History* was published in 1925 at Adelaide. Three years later his classic *History of Trade and Commerce* appeared. In 1934 during the Great Depression he wrote the *British Way to Recovery*, in the war years he contributed to *The Interpretation of History*, he published *Yorkshire Cloth Traders in the United States* in 1944, and in the postwar years he wrote *Socialism in Western Europe*. In 1952 he published *A Scholar in Action*, a tribute to Edwin F. Gay. His greatest work was his *Economic History of Europe*, first published in 1936 and

reprinted in 1948. To this original work he brought new insights, new vistas, new horizons, and a new point of view—all enriched by a questing mind and distinguished by a most gracious literary style. It places him among the great scholars of our generation and will be a lasting memorial.

Herbert Heaton was a good companion. Time spent with him went all too swiftly. One went away stimulated, full of new ideas, new viewpoints, and new challenges. He was exciting, vibrant, serious, and then humorous as the fun of it all struck him. It was good to be with a mind whose boundaries were only those imposed by the limitations of this blue planet and the appearance on it of mankind.

RODNEY C. LOEHR  
*University of Minnesota*

H. DONALDSON JORDAN, professor emeritus of English history at Clark University, died in Worcester, Massachusetts, on December 20, 1972, after a short illness. Besides his membership in the faculty he had served as secretary of the graduate board, chairman of the department of history, government, and international relations, and chairman of the college board, actually dean of the college, a title that characteristically he refused to accept until his last year in office.

Born in Chicago in 1897, he attended schools in that city, Freiburg, Germany, and Geneva, Switzerland. He earned three degrees at Harvard University where he became a teaching fellow and received the Tappan prize for his doctoral dissertation. He became an assistant professor of history at Dartmouth College in 1925, and, after a year abroad as a Guggenheim fellow, accepted an appointment at Clark University in 1931, retiring in 1967.

Dedicated to high standards of scholarship, service, and personal integrity, Professor Jordan practiced long hours, hard work, and self-discipline that, together with a reputation for wise counseling and prudent reflection, earned him an undue amount of committee work and administration. Nevertheless, he never neglected his role as teacher, but rather pursued beyond the call of duty the personal relations with students that a small university made possible. He inspired them with reverence for precision in detail and a shrewd and careful caution in large judgments. Their appreciation, enhanced

rather than diminished by his judicious exercise of the office of dean, won him the title of "Man of the Year" in 1959. The esteem of his colleagues was expressed in his election to Phi Beta Kappa in 1963. Upon his retirement the trustees conferred upon him the degree of L.H.D., a tribute to his years of devotion to the university in a spirit of selfless service rather than that of power hunger or officiousness.

In his busy life of teacher-administrator he coauthored with E. J. Pratt *Europe and the American Civil War* (1931) and contributed to scholarly periodicals, the *Encyclopedia of the Social Sciences*, the *Dictionary of American Biography*, and the *Century Cyclopedic of Names*. At the time of his death he had nearly completed a book on Richard Cobden. A life member of the American Historical Association, he was especially active in the Conference on British Studies.

In addition to his academic life Professor Jordan served in the two world wars. In the first he was an ambulance driver in France, for which he was decorated with the Croix de Guerre, and later was a second lieutenant in the United States infantry. In the second he was an analyst and administrator in the Office of Strategic Services and for a short time served in the Department of State.

His colleagues will remember him not only for his academic leadership and for his broadly based humanism, but also for his warm companionship in both work and play. Despite crippling arthritis he took his part in faculty sports without ever complaining of the pain he often suffered. A teacher-scholar in the best tradition, he maintained a lively interest in the history department after his retirement, continuing to give the benefit of his wisdom to the younger faculty members who often sought him out. Colleagues, former students, and friends are expressing their love and admiration for him by sending a memorial contribution to the H. Donaldson Jordan Memorial Fund, Department of History, Clark University, Worcester, Massachusetts 01610.

DWIGHT E. LEE  
*Clark University*

SHELBY T. McCLOY, University of Kentucky emeritus professor of French history, died at the age of seventy-five in Monticello, Arkansas, on February 15, 1973. A graduate of Davidson Col-

lege, where he received the B.A. in 1918 and the M.A. in 1919, he went on to Oxford University as a Rhodes scholar, taking a B.Litt. and a second B.A. In 1925 he was on the teaching staff at Robert College in Istanbul, an experience to which he often looked back with great fondness. His realization that he was "insufficiently tempered" led him to Columbia University as a Jacob Schiff fellow and to a Ph.D. in 1933. There followed a decade on the faculty of Duke University, and after 1944, until his retirement in 1966, he became a mainstay in the history department at Kentucky.

Professor McCloy was a dignified and quiet man, known for his courteous, almost courtly, manner—a manner that was for him a way of life. His even temper gave way to high dudgeon when he was faced with the slipshod, the unethical, and the unjust. His intellectual cutting edge remained finely honed, even in the last years of his active career when he was forced to battle against a progressive and eventually fatal disease, which he chose to ignore in preference to "more interesting things." He resented most his sudden incapacity to work abroad in what had become his second home, Paris. While he was unable to identify easily the best Parisian restaurants, no one knew the archives more intimately.

He gave himself unstintingly to the University of Kentucky, where he helped to establish the honors program, directed the Rhodes Screening Committee, advised countless numbers of students, directed Ph.D. dissertations, lent a willing ear to the problems of his colleagues, participated in community activities, and with all this, wrote and published extensively. His research interests, centering in seventeenth- and eighteenth-century France, produced fifteen scholarly articles and six books, including *French Inventions of the Eighteenth Century* (1952) and *The Humanitarian Movement in Eighteenth Century France* (1957). In the 1950s, well ahead of present interest in the history of minority groups, he began research on Negro history and in 1961 produced *The Negro in France*. This study then led him to what would be his last book, *The Negro in the French West Indies*, published in 1966.

Professor McCloy's increasing illness forced him into what he and his department viewed as a premature, unwanted, but unavoidable retirement. He took with him the esteem of

the history staff, which had previously recognized his teaching, research, and outstanding service to the university by voting him Hallam Professor of History in 1959–60, and the respect of hundreds of faculty members who in 1960–61 elected him Distinguished Professor of the College of Arts and Sciences, the highest honor they could bestow. The death of this scholar and great gentleman comes hard.

GERARD E. SUBERSTEIN  
University of Kentucky

EARLE DUDLEY ROSS, professor emeritus of history at Iowa State University, died in Ames, Iowa, on March 22, 1973. Dr. Ross was born December 20, 1885, in Ross Hill, Tioga County, New York. A graduate of Syracuse University (1909), he received the doctorate in history at Cornell University in 1915. He taught at Missouri Wesleyan College (1915–17), Simpson College (1917–18), Illinois Wesleyan College (1918–19), and North Dakota Agricultural College (1919–23). Professor Ross joined the faculty of Iowa State College as an associate professor of economic history in 1923, and he retired from active teaching in 1956.

Professor Ross's list of publications is impressive especially because of the pioneering nature of several of his works—studies that are still in high repute among professional historians. The two most significant are *The Liberal Republican Movement*, published in 1919, and *Democracy's College: The Land-Grant Movement in the Formative Stage*, which appeared in 1942. The official historian of Iowa State University, he published *A History of Iowa State College* in 1942, and on the occasion of its centennial in 1958 he contributed *The Land-Grant Idea at Iowa State College*. In addition to these and several other books, he produced forty-four scholarly articles and many book reviews.

What this chronicle of bare facts cannot convey is the great esteem and warm regard that Professor Ross earned from faculty and students as a colleague, as a teacher, and as a man. He was a scholar-teacher in the best sense of the term. His colleagues revered him for his good sense and good temper, his sound judgment, and his personal integrity. His students responded to the example he set and to his kindly yet firm stimulation and encouragement. In the words of the citation read on the oc-

casation of his being awarded the honorary degree of Doctor of Humane Letters at Grinnell College in 1952, Earle D. Ross was a "distinguished member of the faculty of a sister institution, discriminating observer of the American scene; a recorder in brilliant fashion of the economic history of the middle west; and inspiring teacher who has added dignity and luster to the teaching profession."

LOUIS G. GEIGER  
*Iowa State University*

Other members of the association who have died recently include: S. S. Biro of Leucadia, California; Frank J. Cappelluti of Belleville, New Jersey; Robert E. Cleveland of Loma Linda University in Loma Linda, California; C. Doris Hellman of Queens College in New York, New York; Douglass H. McNeally of Portland, Maine; R. S. Penfield of Hartford, Connecticut; J. B. Warden of Pinehurst, North Carolina; and Frank P. Weberg of Joliet, Illinois.



## Other Books Received

Books listed were received by the *AHR* between February 1, and May 1, 1973. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

ADAMS, HENRY H. *Years of Expectation: Guadalcanal to Normandy*. New York: David McKay. 1973. Pp. xiii, 430. \$9.95.

ADLER, CHAIM, *et al.* *World Politics and the Jewish Condition: Essays Prepared for a Task Force on the World of the 1970s of the American Jewish Committee*. Ed., with an introd., by LOUIS HENKIN. [New York:] Quadrangle Books, in collaboration with the Institute of Human Relations Press. 1972. Pp. x, 342. \$9.95.

ALBERIGO, GIUSEPPE, *et al.* *Legge e Vangelo: Discussione su una legge fondamentale per la Chiesa*. Testi e ricerche di Scienze religiose, 8. Brescia: Paideia Editrice. 1972. Pp. 712. L. 7,000.

ALBERIGO, JOSEPHO, *et al.* (eds.). *Conciliorum Oecumenicorum Decreta*. 3d ed.; Bologna: Istituto per le scienze religiose. 1973. Pp. xxiv, 1135, 169.

ARAI, TSUGUO (ed.). *Alaska and Japan: Perspectives of Past and Present*. [Anchorage:] Alaska Methodist University Press. 1972. Pp. 172.

BAINTON, ROLAND H., and GRITSCH, ERIC W. *Bibliography of the Continental Reformation: Materials Available in English*. 2d rev. ed.; [Hamden, Conn.]: Archon Books. 1972. Pp. xix, 220. \$10.00.

BERGER, PETER L., *et al.* *The Homeless Mind: Modernization and Consciousness*. New York: Random House. 1973. Pp. xii, 258. \$6.95.

*Bibliographie internationale de l'Humanisme et de la Renaissance*. Vol. 6, *Travaux parus en 1970*. Fédération internationale des Sociétés et Instituts pour l'étude de la Renaissance. Ouvrage publié sur la recommandation du Conseil International de la Philosophie et des Sciences Humaines, avec le concours du C.N.R.S. et de l'U.N.E.S.C.O. Geneva: Librairie Droz. 1973. Pp. xii, 678.

BLACKBURN, ROBIN (ed.). *Ideology in Social Science: Readings in Critical Social Theory*. New York: Pantheon Books. 1972. Pp. 382. Cloth \$8.95, paper \$2.95.

BLOCH, MARC. *The Royal Touch: Sacred Monarchy and Scrofula in England and France*. Tr. by J. E. ANDERSON. Montreal: McGill-Queen's University Press. 1973. Pp. xi, 441. \$18.95. See rev. of French ed. (1924), *AHR*, 30 (1924-25): 584.

BLOOMFIELD, MASSE, and WOLF, HARVEY J. *Man in Transition: A Concept of History*. Reseda, Calif.: Mojave Books. 1973. Pp. vii, 87. \$8.00.

BRODIE, BERNARD. *War and Politics*. New York: Macmillan. 1973. Pp. xii, 514. \$8.95.

BRODIE, BERNARD and FAWN M. *From Crossbow to H-Bomb*. Rev. ed.; Bloomington: Indiana University Press. 1973. Pp. 320. Cloth \$7.95, paper \$2.95.

BROWN, W. NORMAN. *The United States and India, Pakistan, Bangladesh*. The American Foreign Policy Library. 3d ed.; Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 462. \$16.00. See rev. of 1st ed. (1953), *AHR*, 59 (1953-54): 175.

BUTTS, R. FREEMAN. *The Education of the West: A Formative Chapter in the History of Civilization*. New York: McGraw-Hill. 1973. Pp. xvi, 631. \$10.95.

CARTER, GWENDOLEN M., and HERZ, JOHN H. *Government and Politics in the Twentieth Century*. 3d ed.; New York: Praeger. 1973. Pp. xiii, 278. \$9.00.

CONNELLY, OWEN. *The Epoch of Napoleon*. Berkshire Studies in History. New York: Holt, Rinehart and Winston. 1972. Pp. viii, 198.

CONOVER, PAUL H. (ed.). *Asia and Africa: Introductory Studies of Non-Western Societies*. Columbus, Ohio: Charles E. Merrill. 1973. Pp. vi, 249. \$4.95.

COOPER, JOHN MILTON, JR. (ed. with an introd.). *Causes and Consequences of World War I*. New York: Quadrangle Books. 1972. Pp. 360.

CROSBY, EVERETT UBERTO, and WEBB, CHARLES R., JR. (eds.). *The Past as Prologue: Sources and Studies in European Civilization*. In 2 vols. New York: Appleton-Century-Crofts. 1973. Pp. x, 498; x, 604.

DUDLEY, GUILFORD A., *et al.* *A History of World Civilizations*. Ed. by EDWARD R. TANNENBAUM. New York: John Wiley and Sons. 1973. Pp. xv, 925. \$13.95.

EYCK, FRANK (ed.). *The Revolutions of 1848-49*. New York: Barnes and Noble. 1972. Pp. viii, 202. Cloth \$10.50, paper \$5.50.

- FLOUDD, RODERICK. *An Introduction to Quantitative Methods for Historians*. Princeton: Princeton University Press. 1973. Pp. ix, 220. \$7.50.
- GERBI, ANTONELLO. *The Dispute of the New World: The History of a Polemic, 1750-1900*. Tr. by JEREMY MOYLE. Rev. ed.; [Pittsburgh:] University of Pittsburgh Press. 1973. Pp. xviii, 700. \$19.95. See rev. of Italian ed. (1955), *AHR*, 62 (1956-57): 602.
- GILLISPIE, CHARLES COULSTON (ed. in chief). *Dictionary of Scientific Biography*. Vol. 6. *Jean Hachette-Joseph Hyrtl*. Pub. under the auspices of the American Council of Learned Societies. New York: Charles Scribner's Sons. 1972. Pp. xiii, 619. \$35.00. See rev. of vols. 1 and 2 (1970), *AHR*, 78 (1973): 64.
- GIURESCU, CONSTANTIN C. *On Romanian-American Cultural Relations*. New York: Romanian Library. [1972.] Pp. 20.
- HACHEY, THOMAS F. (ed.). *Anglo-Vatican Relations, 1914-1939: Confidential Annual Reports of the British Ministers to the Holy See. Her Majesty's Stationery Office, London, England*. Boston: G. K. Hall. 1972. Pp. xli, 403. \$18.00.
- HEATER, D. B. *Political Ideas in the Modern World*. 4th ed. rev.; New York: Barnes and Noble. 1972. Pp. 230. \$2.75.
- HERMANN, CHARLES F. (ed.). *International Crises: Insights from Behavioral Research*. New York: Free Press. 1972. Pp. x, 334. \$12.95.
- HURST, MICHAEL (selected and ed.). *Key Treaties for the Great Powers, 1814-1914*. Vol. 1, 1814-1870; vol. 2, 1871-1914. New York: St. Martin's Press. 1972. Pp. xviii, 458; ix, 459-948. \$31.50 the set.
- IONESCU, GHITA, and DE MADARIAGA, ISABEL. *Opposition: Past and Present of a Political Institution*. Pelican Book. [Baltimore:] Penguin Books. 1972. Pp. 200. \$1.95.
- JAL, D'AUGUSTIN. *Nouveau glossaire nautique*. B. Rev. ed.; Paris: Mouton. 1972. Pp. 53-166.
- KASLAS, BRONIS J. (ed., with a foreword and introd.). *The USSR-German Aggression against Lithuania*. New York: Robert Speller and Sons. 1973. Pp. 543. \$15.00.
- KING, JFRE CLEMENS (ed.). *The First World War. The Documentary History of Western Civilization*. New York: Walker. 1972. Pp. xlvii, 350. \$12.50.
- LLOYD, CHRISTOPHER. *The Nile Campaign: Nelson and Napoleon in Egypt*. Illustrated Sources of History. New York: Barnes and Noble. 1973. Pp. 120. \$8.50.
- LUBASZ, HEINZ (ed.). *Fascism: Three Major Regimes. Major Issues in History*. New York: John Wiley and Sons. 1973. Pp. viii, 188. Cloth \$7.50, paper \$4.50.
- LUMER, HYMAN. *Zionism: Its Role in World Politics*. New York: International Publishers. 1973. Pp. 154. \$2.45.
- MACFARLANE, L. J. *Modern Political Theory*. New York: Barnes and Noble. 1973. Pp. xxv, 269. Cloth \$8.75, paper \$4.25.
- MAUMGREN, HARALD B. *International Economic Peacekeeping in Phase II*. Rev. ed.; [New York:] Quadrangle Books, for the Atlantic Council of the United States. 1973. Pp. xvi, 276. \$3.95.
- MARCUSE, HERBERT. *Studies in Critical Philosophy*. Tr. by JORIS DE BRES. Boston: Beacon Press. 1972. Pp. 227. \$7.95.
- MARX, KARL, and ENGELS, FREDERICK. *The Revolution of 1848-49: Articles from the Neue Rheinische Zeitung*. New York: International Publishers. 1972. Pp. 302. Cloth \$7.50, paper \$2.25.
- MCCUTCHEON, JAMES M. (comp.). *China and America: A Bibliography of Interactions, Foreign and Domestic*. East-West Bibliographic Ser., 1. Honolulu: University Press of Hawaii. 1972. Pp. x, 75. \$4.50.
- MEINDE, TIBOR. *From Aid to Re-colonization: Lessons of a Failure*. New York: Pantheon Books. 1973. Pp. xxix, 317. \$10.00.
- MEYERS, EDMUND D., JR. *Time-Sharing Computation in the Social Sciences*. Prentice-Hall Ser. in Automatic Computation. Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. xiv, 409. \$10.95.
- MUMFORD, LEWIS. *Interpretations and Forecasts: 1922-1972. Studies in Literature, History, Biography, Technics, and Contemporary Society*. New York: Harcourt Brace Jovanovich. 1973. Pp. ix, 522. \$12.95.
- NISBET, ROBERT. *The Social Philosophers: Community and Conflict in Western Thought*. New York: Thomas Y. Crowell. 1973. Pp. xii, 466. \$10.00.
- NOWAK, ZDZISLAW. *Procesy dezintegracji i integracji ekonomicznej we współczesnym kapitalizmie* [Processes of Economic Integration and Disintegration in Contemporary Capitalism]. *Prace Instytutu Zachodniego*, No. 44. Poznań: Instytut Zachodni. 1972. Pp. 229. Zł. 45.
- NYE, ROBERT D. *Conflict among Humans: Some Basic Psychological and Social-Psychological Considerations*. New York: Springer. 1973. Pp. xv, 205. Cloth \$7.50, paper \$4.50.
- PADFIELD, PETER. *The Battleship Era*. New York: David McKay. 1972. Pp. 321. \$9.95.
- POLLARD, SIDNEY, and HOLMES, COLIN. *Industrial Power and National Rivalry, 1870-1914. Documents of European Economic History*, vol. 2. New York: St. Martin's Press. 1972. Pp. xv, 509. \$21.95.
- RANDLE, ROBERT F. *The Origins of Peace: A Study of Peacemaking and the Structure of Peace Settlements*. New York: Free Press. 1973. Pp. xv, 550. \$12.95.
- ROBSON, F. R. *School Architecture*. With an introd. by MALCOLM SEABORNE. The Victorian Library. Reprint; New York: Humanities Press. 1972. Pp. 440. \$14.75.
- ROSOVSKY, HENRY (ed.). *Discord in the Pacific: Challenges to the Japanese-American Alliance*. The American Assembly. Columbia University. Washington: Columbia Books. 1972. Pp. 251. \$3.00.
- SALIS, J.-R. DE. *Switzerland and Europe: Essays and Reflections*. Tr. from the German by ALEXANDER and ELIZABETH HENDERSON. Introd. by CHRISTOPHER

HUGHES. University of Alabama Press. [1973.] Pp. 319. \$7.50.

SUMLER, DAVID E. *A History of Europe in the Twentieth Century*. The Dorsey Ser. in European History. Homewood, Ill.: Dorsey Press. 1973. Pp. xiii, 516. \$7.25.

TRIVICK, HENRY. *The Picture Book of Brasses in Gilt*. New York: Charles Scribner's Sons. 1971. Pp. 33, 242 plates. \$15.00.

TUNNEY, CHRISTOPHER. *A Biographical Dictionary of World War II*. New York: St. Martin's Press. 1972. Pp. viii, 216. \$8.95.

VIAL, JEAN. *L'avènement de la civilisation industrielle de 1815 à nos jours*. Collection SUP. L'historien, 13. [Paris:] Presses Universitaires de France. 1973. Pp. 230.

WEINER, MARGERY. *The Sovereign Remedy: Europe after Waterloo*. New York: St. Martin's Press. 1973. Pp. xii, 272. \$8.95.

WHITAKER, BEN (ed.). *The Fourth World: Victims of Group Oppression*. Eight reports from the field work of the Minority Rights Group. New York: Schocken Books. 1973. Pp. 342. \$10.00.

WOODBURY, ROBERT S. *Studies in the History of Machine Tools*. Reprint; Cambridge, Mass.: M.I.T. Press. 1972. Pp. iv, 135, 191, 107, 124. \$4.95. See revs. of *History of the Gear-Cutting Machine* (1958) and *History of the Grinding Machine* (1959), *AHR*, 65 (1959-60): 944; *History of the Milling Machine* (1960), *AHR*, 66 (1960-61): 186, and *History of the Lathe to 1850* (1961), *AHR*, 68 (1962-63): 170.

#### ANCIENT

CLARKE, DAVID L. (ed.). *Models in Archaeology*. London: Methuen; distrib. by Barnes and Noble. New York. 1972. Pp. xxiv, 1955. \$62.50.

FERGUSON, JOHN. *The Heritage of Hellenism: The Greek World from 323 BC to 31 BC*. History of European Civilization Library. New York: Harcourt Brace Jovanovich. 1973. Pp. 180. \$3.95.

MARTIN, JEAN-PIERRE. *La Rome ancienne: 753 avant J.-C./395 après J.-C. Le fil des temps*, 2. [Paris:] Presses Universitaires de France. 1973. Pp. 351.

SCHMIDT, JOHN D. *Ramesses II: A Chronological Structure for His Reign*. The Johns Hopkins Near Eastern Studies. Baltimore: Johns Hopkins University Press. 1973. Pp. vii, 216. \$10.00.

#### MEDIEVAL

BUCK, GEORGE. *The History of the Life and Reign of Richard the Third*. With a new introd. by A. R. MYERS. Reprint; Totowa, N.J.: Rowman and Littlefield. 1973. Pp. ix, 150. \$10.00.

DEMM, FERHARD. *Reformmönchtum und Slawenmission im 12. Jahrhundert: Wertsoziologisch-geistesgeschichtliche Untersuchungen zu den Fälen Bischof Ottos von Bamberg*. Historische Studien, no. 419. Lübeck: Matthiesen Verlag. 1970. Pp. 214. DM 32.

FOURQUIN, GUY. *Les soulèvements populaires au Moyen Age*. Collection SUP, L'historien, 12. [Paris:] Presses Universitaires de France. 1972. Pp. 216. 15 fr.

FRITZ, WOLFGANG D. (ed.). *Die Goldene Bulle Kaiser Karls IV. vom Jahre 1356: Text*. Fontes Iuris Germanici Antiqui, no. 11. Weimar: Hermann Böhlhaus Nachfolger. 1972. Pp. 105. DM 14.80.

MANEGOLD VON LAUTENBACH. *Liber contra Wolfellum*. Ed. by WILFRIED HARTMANN. Monumenta Germaniae Historica: Die deutschen Geschichtsquellen des Mittelalters, 500-1500. Quellen zur Geistesgeschichte des Mittelalters, no. 8. Weimar: Hermann Böhlhaus Nachfolger. 1972. Pp. 133. DM 18.

NICHOLAS, DAVID. *The Medieval West, 400-1450: A Preindustrial Civilization*. The Dorsey Ser. in European Civilization. Homewood, Ill.: Dorsey Press. 1973. Pp. xiv, 291. \$4.50.

OIKONOMIDÈS, NICOLAS (introd., text, tr., and commentary). *Les listes de présence byzantines des IX<sup>e</sup> et X<sup>e</sup> siècles*. Le monde byzantin. Paris: Éditions du Centre national de la recherche scientifique. 1972. Pp. 403. 120.40 fr.

PIRZANOWSKI, ZBIGNIEW (ed.). *Dokumenty sądu ziemskiego krakowskiego, 1302-1453* [Documents of the Cracow Land Court, 1302-1453]. Polska Akademia Nauk—Oddział w Krakowie. Materiały Komisji Nauk Historycznych, no. 20. Cracow: the Akademia. 1971. Pp. lxvi, 314. Zł. 70.

SCHLIGHT, JOHN. *Henry II Plantagenet*. Twayne's Rulers and Statesmen of the World Ser., 19. New York: Twayne. 1973. Pp. 219.

TREVELYAN, GEORGE MACAULAY. *England in the Age of Wycliffe*. With an introd. by J. A. TUCK. Reprint; [London:] Longman; distrib. by Rowman and Littlefield, Totowa, N.J. 1972. Pp. xlviii, 380. \$4.00. See rev. of 1st ed. (1899), *AHR*, 5 (1899-1900): 120.

ULLMANN, WALTER. *The Future of Medieval History: An Inaugural Lecture*. New York: Cambridge University Press. 1973. Pp. 30. \$1.45.

#### BRITISH COMMONWEALTH AND IRELAND

BECKETT, J. C. *Confrontations: Studies in Irish History*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 175. \$10.00.

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#### A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible, articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

<i>Abt</i>	Abteilung	<i>archaeol</i>	archaeolog, archaeology
<i>acad</i>	academia, academy	<i>archäol</i>	Archäologie, archäologische,
<i>accad</i>	accademia		archäologischer
<i>adm</i>	administration, administrative	<i>archeol</i>	archeologia, archeologie, archeology
<i>aff</i>	affairs	<i>Ariz</i>	Arizona
<i>afric</i>	africain, African	<i>Ark</i>	Arkansas
<i>afrik</i>	afrikaanse	<i>ark</i>	arkiv
<i>agric</i>	agricultural, agriculture	<i>arq</i>	arquivos
<i>AHR</i>	<i>American Historical Review</i>	<i>arqueol</i>	arqueológico
<i>aikakausk</i>	aikakauskirja	<i>art</i>	article
<i>Akad</i>	Akademie	<i>assoc</i>	association
<i>Ala</i>	Alabama	<i>assyriol</i>	assyriological, assyriologie, assyriology
<i>Alas</i>	Alaska	<i>at</i>	atti
<i>alemann</i>	alemannisches	<i>Atl</i>	Atlantic
<i>allg</i>	allgemein	<i>AUMLA</i>	<i>Journal of the Australasian Universities</i>
<i>Altertumsk</i>	Altertumskunde		<i>Language and Literature Association</i>
<i>alttest</i>	alttestamentliche	<i>autobiog</i>	autobiography
<i>Am</i>	American, Americana, Amerikas		
<i>an</i>	anales, Annalen, annales, annali, annals, annua, annuaires, annual, annuarius	<i>b</i>	buch (compounds only)
<i>anc</i>	ancien, ancient	<i>balt</i>	baltisch
<i>annot</i>	annotation	<i>bayer</i>	bayerisch
<i>anthol</i>	anthologica, anthology	<i>Beitr</i>	Beitrag, Beiträge
<i>anthropol</i>	anthropologie, anthropology	<i>Ber</i>	Bericht
<i>antiq</i>	antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity	<i>bibl</i>	bibliotek, bibliotheca, bibliothèque
		<i>bibliogr</i>	bibliografice, bibliographical, bibliography
		<i>byd</i>	bijdragen
		<i>biog</i>	biography
<i>antol</i>	antologia	<i>Bl</i>	Blatt, Blätter
<i>antropol</i>	antropologiczny	<i>BMGN</i>	<i>Bydragen en Mededelingen betreffende de</i>
<i>anz</i>	anzeiger		<i>Geschiedenis der Nederlanden</i>
<i>appenzell</i>	appenzellische	<i>bol</i>	boletim, boletin
<i>arch</i>	archiven, archives, archivio, archivo, archiv, Archivum	<i>boll</i>	bollettino

<i>brandenburg</i>	brandenburgisch	<i>Eur</i>	Europas, Europe, European, européennes
<i>bras</i>	brasileira	<i>ev</i>	evangelisch
<i>braunschw</i>	braunschweigisch	<i>explor</i>	explorations
<i>Braz</i>	Brazilian		
<i>brem</i>	bremisches	<i>fac</i>	faculté, faculty
<i>Brit</i>	British	<i>facs</i>	facsimile
<i>bull</i>	bulletin	<i>Fak</i>	Fakulte
<i>bus</i>	business	<i>fil</i>	filosofia, filozofski, filozofskog
<i>byz</i>	byzantine	<i>filol</i>	filologia
		<i>Fla</i>	Florida
<i>cah</i>	cahiers	<i>for</i>	foreign
<i>Calif</i>	California	<i>Forsch</i>	Forschung, Forschungen
<i>Can</i>	Canadian	<i>fr</i>	français, French
<i>Carib</i>	Caribbean	<i>francisc</i>	franciscanum
<i>cath</i>	catholic	<i>fränk</i>	fränkische
<i>cent</i>	century	<i>frankf</i>	frankfurter
<i>cercet</i>	cercetări		
<i>českoslov</i>	československý	<i>g</i>	giornale
<i>chron</i>	chronicles, chronique	<i>Ga</i>	Georgia
<i>circ</i>	circle, circular	<i>gaz</i>	gazette
<i>civil</i>	civilization	<i>gen</i>	general, général
<i>class</i>	classica, classical, classique	<i>geneal</i>	genealogy
<i>co</i>	county	<i>geog</i>	geográfico, geographic, geographical, géographique, geographischen, geography
<i>coll</i>	college		
<i>collect</i>	collection, collections	<i>Ger</i>	German
<i>Colo</i>	Colorado	<i>germ</i>	germanistisch
<i>com</i>	comité, committee	<i>Ges</i>	Gesellschaft
<i>comm</i>	commerce	<i>gesch</i>	Geschichte, geschichtliche
<i>comp</i>	comparate, comparative	<i>gos</i>	gospodarczych
<i>compil</i>	compilation, compiled, compiler	<i>govt</i>	government
<i>concl</i>	conclusion	<i>grad</i>	graduate
<i>conf</i>	conference	<i>Grafsch</i>	Grafschaft
<i>cong</i>	congress		
<i>Conn</i>	Connecticut	<i>h</i>	hefte (compounds only)
<i>contemp</i>	contemporaine, contemporânea, contemporary	<i>hamburg</i>	hamburgisch
<i>corp</i>	corporation	<i>hann</i>	hannoversche
<i>corr</i>	correspondence	<i>hell</i>	hellenic, hellénique, hellenistic
<i>c. r.</i>	comptes rendus	<i>helvet</i>	helvetian
<i>crit</i>	critica, criticism	<i>hess</i>	hessisch
<i>cuad</i>	cuaderno	<i>Hi</i>	Hawaii
<i>cult</i>	cultura, cultural, culture	<i>hisp</i>	hispanic, hispánicos
		<i>hist</i>	histoire, historiae, historialinen, historical, historická, historický, histórico, historicum, historique, historisch, history, historyczne
<i>D.C.</i>	District of Columbia	<i>hohenzoll</i>	hohenzollerische
<i>Del</i>	Delaware	<i>holstein</i>	holsteinisch
<i>demog</i>	demografie, demographische, demography		
<i>dept</i>	department	<i>iaz</i>	iazyka
<i>deux</i>	deuxième	<i>Ida</i>	Idaho
<i>dev</i>	developing, development	<i>Ill</i>	Illinois
<i>dig</i>	digest	<i>illus</i>	illustrated
<i>dipl</i>	diplomatic, diplomatique	<i>ind</i>	industrial, industry
<i>doc</i>	documentation, documents	<i>Inda</i>	Indiana
<i>dok</i>	dokuments	<i>individ</i>	individual
<i>drev</i>	drevnei	<i>inst</i>	Institut, institute, institution, instituto
<i>dtisch</i>	deutsche, deutschen, deutsches	<i>int</i>	internacional, international, internationale
<i>e</i>	east, eastern	<i>interdisc</i>	interdisciplinary
<i>ec</i>	economics, économique, economy	<i>intern</i>	internal
<i>eccles</i>	ecclesiastical	<i>introd</i>	introduced, introduction
<i>ecclcs</i>	eclesiástico	<i>ist</i>	istorii, istorijski, istoriski
<i>ed</i>	edited, edition, editor	<i>istruz</i>	istruzione
<i>educ</i>	education	<i>ital</i>	Italian, italiana, italienisch
<i>EEH</i>	<i>Explorations in Economic History</i>		
<i>EHR</i>	<i>English Historical Review</i>	<i>j</i>	journal
<i>ELH</i>	<i>English Literary History</i>	<i>jb</i>	Jahrbuch, Jahrbücher
<i>Eng</i>	English	<i>JEGP</i>	<i>Journal of English and Germanic Philology</i>
<i>epig</i>	epigraphik, epigraphy		
<i>Épis</i>	Episcopal	<i>jugoslov</i>	jugoslovenski
<i>estud</i>	estudios	<i>jur</i>	juridical, juridiceski, juridique
<i>et</i>	études		
<i>ethnog</i>	ethnographisch		
<i>ethnol</i>	ethnological, ethnology		
<i>etnol</i>	etnologia		

<i>kan</i>	kanonistisch	<i>oesterr</i>	oesterreichisch
<i>Kans</i>	Kansas	<i>ok</i>	økonomie
<i>kath</i>	katholik	<i>Okla</i>	Oklahoma
<i>Kd</i>	Kunde	<i>Ore</i>	Oregon
<i>Kl</i>	Klasse	<i>organ</i>	organization
<i>Ky</i>	Kentucky	<i>orient</i>	oriental, orientale, orientalia, orientalistyczny
<i>La</i>	Louisiana	<i>österr</i>	österreichisch
<i>Landesk</i>	Landeskunde	<i>Osth</i>	Osthefte
<i>lang</i>	language	<i>Pa</i>	Pennsylvania
<i>lett</i>	letter, letterário, letterature, lettre	<i>Pac</i>	Pacific
<i>lib</i>	library	<i>pädagog</i>	pädagogik, pädagogisch
<i>Lib Cong</i>	U.S. Library of Congress	<i>paedagog</i>	paedagogica
<i>libr</i>	librarian	<i>pap</i>	papers
<i>ling</i>	linguistics, linguistique	<i>papyrol</i>	papyrologie
<i>lit</i>	literary, literatur, literature, literatury	<i>parl</i>	parlementaire, parlement
<i>lübeck</i>	lübeckische	<i>pfälz</i>	pfälzische
<i>lüneburg</i>	lüneburger	<i>phil</i>	philosophical, philosophique, philosophy
<i>mag</i>	magasin, magazine	<i>philol</i>	philology
<i>marit</i>	maritime	<i>photo</i>	photograph
<i>Mass</i>	Massachusetts	<i>pol</i>	political, político, politics, Politik, politique, politische
<i>Md</i>	Maryland	<i>port</i>	portuguesa, portuguese
<i>Me</i>	Maine	<i>pres</i>	president, presidential
<i>med</i>	medieval, médiévale, medievals	<i>Presb</i>	Presbyterian
<i>meded</i>	mededelingen	<i>preuss</i>	preussisch
<i>Mediterr</i>	Mediterranean	<i>probl</i>	problems
<i>mél</i>	mélanges	<i>proc</i>	proceedings
<i>mém</i>	mémoires, memorial, memorie	<i>prot</i>	protestant, Protestantismus
<i>mennonit</i>	mennonitische	<i>prov</i>	providence, provinces
<i>Mex</i>	Mexican	<i>PSQ</i>	<i>Political Science Quarterly</i>
<i>Mich</i>	Michigan	<i>psych</i>	psychology
<i>mid</i>	middle	<i>pt</i>	part
<i>midcont</i>	midcontinental	<i>pts</i>	parts
<i>mil</i>	militaire, militarisch, military	<i>publ</i>	publication, publishing
<i>Minn</i>	Minnesota	<i>q</i>	quaderni, quarterly
<i>misc</i>	miscelánea, miscellany	<i>quel</i>	quellen
<i>Miss</i>	Mississippi	<i>r</i>	review, revista, revue, rivista
<i>Mitt</i>	Mitteilung, Mitteilungen	<i>rass</i>	rassegna
<i>Mo</i>	Missouri	<i>Rdsch</i>	Rundschau
<i>mod</i>	modern, moderna, moderne	<i>rec</i>	record
<i>mond</i>	mondiale	<i>rech</i>	recherches
<i>Mont</i>	Montana	<i>regist</i>	register
<i>monum</i>	monumenta	<i>relig</i>	religion, religiöse, religious
<i>movim</i>	movimento	<i>rend</i>	rendiconti
<i>mt</i>	mountain	<i>rep</i>	report, reporter
<i>mus</i>	musei, museum	<i>res</i>	research
<i>n</i>	north, northern	<i>rev</i>	revolution
<i>nac</i>	nacional	<i>rhein</i>	rheinisch
<i>nass</i>	nassauische	<i>R.I.</i>	Rhode Island
<i>nat</i>	national	<i>ric</i>	ricerche
<i>nationalok</i>	nationaløkonomie, nationaløkonomisk	<i>rocz</i>	roczniki
<i>naz</i>	nazionale	<i>röm</i>	römische
<i>N.C.</i>	North Carolina	<i>roman</i>	romanische
<i>N.D.</i>	North Dakota	<i>roy</i>	royal
<i>ne</i>	northeast	<i>s</i>	south, southern
<i>Nebr</i>	Nebraska	<i>S.C.</i>	South Carolina
<i>neutest</i>	neutestamentliche	<i>Scand</i>	Scandinavia, Scandinavian
<i>Nev</i>	Nevada	<i>schles</i>	schlesisch
<i>newslett</i>	newsletter	<i>Schr</i>	Schrift
<i>N.H.</i>	New Hampshire	<i>schweiz</i>	schweizerisch
<i>niedersächs</i>	niedersächsisch	<i>sci</i>	science, scientiarum, scientific, scientist, scienze
<i>N.J.</i>	New Jersey	<i>S.D.</i>	South Dakota
<i>N.M.</i>	New Mexico	<i>se</i>	southeast
<i>no</i>	number	<i>sec</i>	sectio, section
<i>nos</i>	numbers	<i>ser</i>	série, series
<i>Nor</i>	Norway	<i>slaw</i>	slawistik
<i>nord</i>	nordisk	<i>soc</i>	social, societatis, society
<i>norm</i>	normale		
<i>numis</i>	numismatic, numismatique		
<i>nw</i>	northwest		
<i>N.Y.</i>	New York		

<i>sociog</i>	sociographiques	<i>Va</i>	Virginia
<i>sociol</i>	sociologia, sociological, sociology	<i>vaterl</i>	vaterlandisch
<i>solothurn</i>	solothurnische	<i>ver</i>	Verein, vereinigung, Vereins
<i>sozial</i>	sozialistischen	<i>Verh</i>	Verhandlungen
<i>Soziol</i>	Soziologie	<i>Veröff</i>	Veröffentlichungen
<i>Span</i>	Spanish	<i>vesn</i>	vesnik
<i>spøl</i>	spółeczność	<i>vest</i>	vestnik
<i>stat</i>	statistical, statistics, Statistik	<i>volksk</i>	volkskunde
<i>stift</i>	stiftung	<i>vopr</i>	voprosy
<i>stor</i>	storia, storici, storico	<i>vrem</i>	vremennuk
<i>stud</i>	studi, studia, Studien, studies, studium	<i>Vt</i>	Vermont
<i>sup</i>	superiore		
<i>suppl</i>	supplement		
<i>sw</i>	southwest		
<i>Sw</i>	Sweden	<i>w</i>	west, western
<i>Swed</i>	Swedish	<i>Wash</i>	Washington
<i>symp</i>	symposium	<i>westf</i>	westfälisch
		<i>wirtsch</i>	Wirtschaft, wirtschaftlich
<i>tech</i>	technisch	<i>Wis</i>	Wisconsin
<i>technol</i>	technology	<i>wiss</i>	Wissenschaft, wissenschaftlich
<i>Tenn</i>	Tennessee	<i>WMQ</i>	<i>William and Mary Quarterly</i>
<i>test</i>	testament, testamentum	<i>württemb</i>	württembergisch
<i>Tex</i>	Texas	<i>W. Va.</i>	West Virginia
<i>theol</i>	theological, theology	<i>Wyo</i>	Wyoming
<i>tids</i>	tidskrift, tidsskrift		
<i>tijd</i>	tijdschrift		
<i>tr</i>	translated, translation, translator	<i>yrbk</i>	yearbook
<i>trans</i>	transactions		
<i>trav</i>	travail, travaux		
<i>u</i>	und	<i>Z</i>	Zeitschrift, Zeitschriften
<i>U</i>	Universitătii, University	<i>Zeitgesch</i>	Zeitgeschichte
<i>Unter</i>	Unterricht	<i>zgodov</i>	zgodovinski
		<i>zhurn</i>	zhurnal





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# The American Historical Association

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Founded in 1884. Chartered by Congress in 1889

Office: 400 A Street, S.E., Washington, D.C. 20003

President: Lynn White, jr., *University of California, Los Angeles*

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**MEMBERSHIP:** Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 18,000. Members elect the officers by ballot.

**MEETINGS:** The Association's annual meeting takes place on December 28–30. The meeting in 1973 will be held in San Francisco. Many professional historical groups meet within or jointly with the Association at this time. Except for 1973 when it will meet with the AHA, the Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report* (copies available on request at a cost of fifty cents), the *AHA Newsletter*, a variety of pamphlets on historical subjects, and bibliographical and other volumes. To promote history and assist historians, the Association offers many other services. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

**PRIZES:** The *Herbert B. Adams Prize* of \$300 awarded annually for a first book in the field of European history. The *Troyer Steele Anderson Prize* awarded every ten years to the person whom the Council of the Association considers to have made the most outstanding contribution to the advancement of the purposes of the Association during the preceding ten years (next award, 1980). The *George Louis Beer Prize* of \$300 awarded annually for a first book on any phase of European international his-

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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Secretary at 400 A Street, S.E., Washington, D.C. 20003.

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# The American Historical Review

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Founded in 1895

The *AHR* is sent to all members of the American Historical Association; information concerning membership will be found on the preceding page. The *AHR* is also available to institutions by subscription. There are two categories of subscription:

CLASS I: *American Historical Review* only, United States, Canada, and Mexico \$25.00, foreign \$27.00.

CLASS II: *American Historical Review*, the *AHA Newsletter*, and the program of the annual meeting of the Association, United States, Canada, and Mexico \$30.00, foreign \$32.00.

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Correspondence regarding contributions to the *American Historical Review* and books for review should be sent to the Editor, 400 A Street, S.E., Washington, D.C. 20003. Unsolicited book reviews or review articles are not accepted; a statement concerning reviewing policy will be found in the issue for December 1970, vol. 75, pp. 1889-91. Preliminary inquiries concerning articles are not necessary, though authors may find them useful. Attention is urged, however, to a statement concerning the kinds of articles the *AHR* ordinarily will and will not publish; it appears in the issue for October 1970, vol. 75, pp. 1577-80. The entire text, including footnotes, of manuscripts submitted for publication must be prepared in double-spaced typescript, with generous margins to allow for copyediting. Footnotes should be numbered consecutively throughout and should appear in a separate section at the end of the text. The editors of the *AHR* are the final arbiters of length, grammar, usage, and the laws of libel; articles will be edited to conform to *AHR* style in matters of punctuation, capitalization, and the like. The editors may suggest other changes in the interests of clarity and economy of expression, but such changes are not made without consultation with authors. There is no official style sheet for the *American Historical Review*, but a convenient general guide is *A Manual of Style*, published by the University of Chicago Press.

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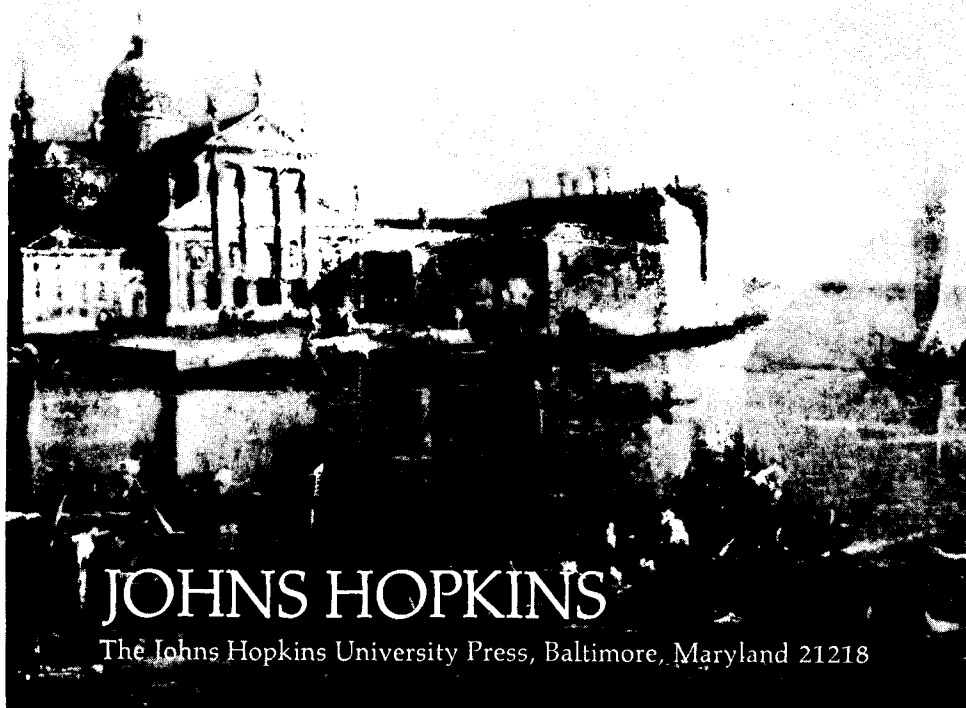


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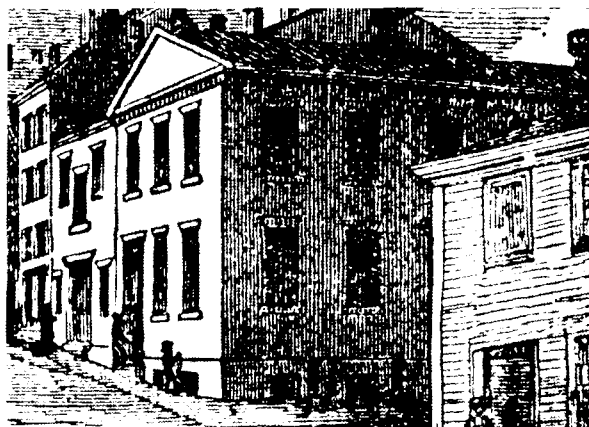
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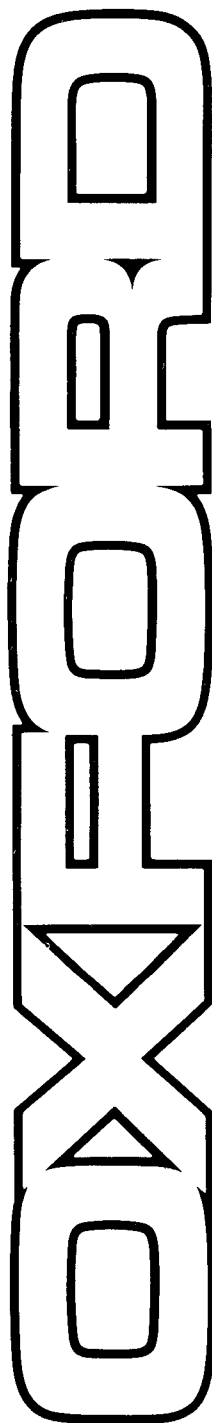
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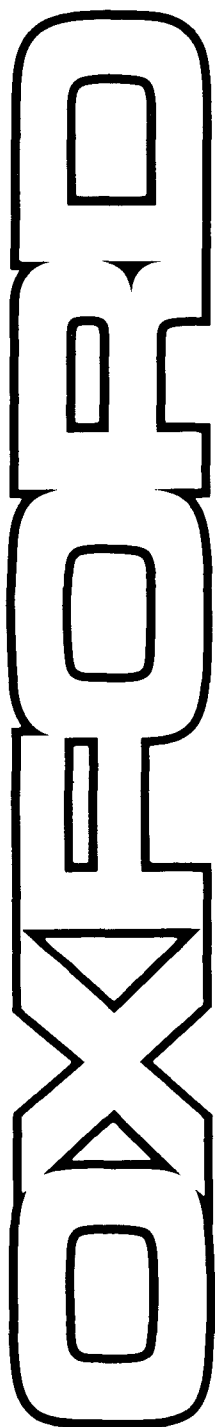
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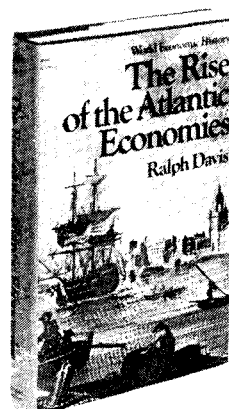
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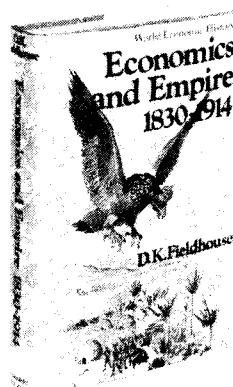


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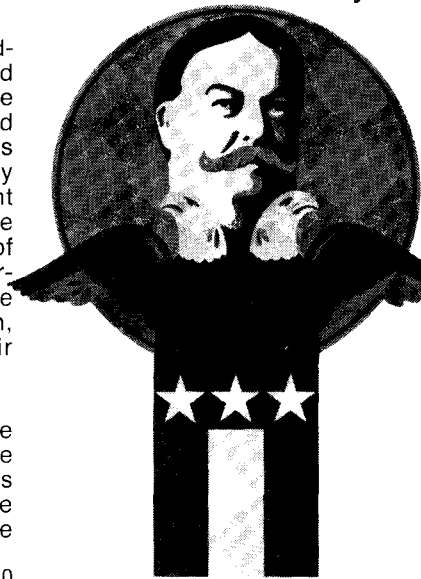
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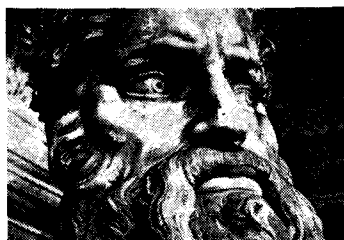
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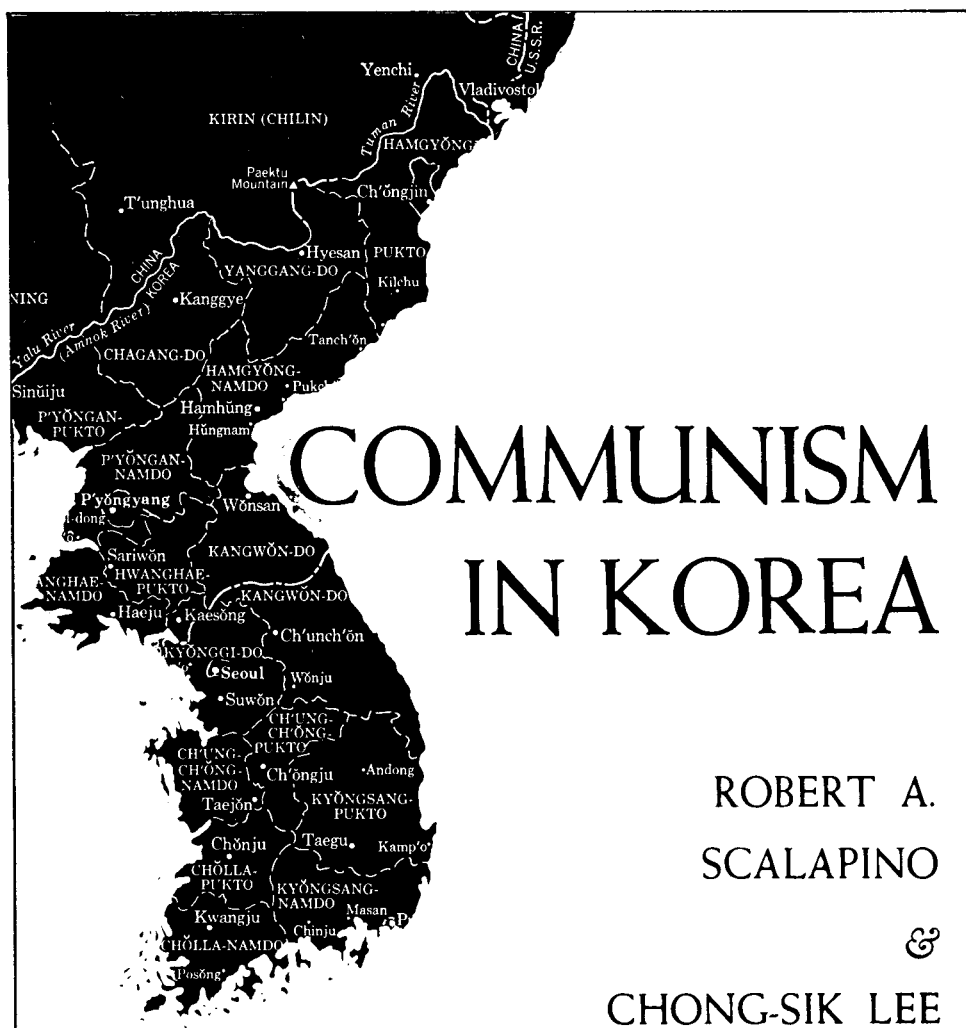
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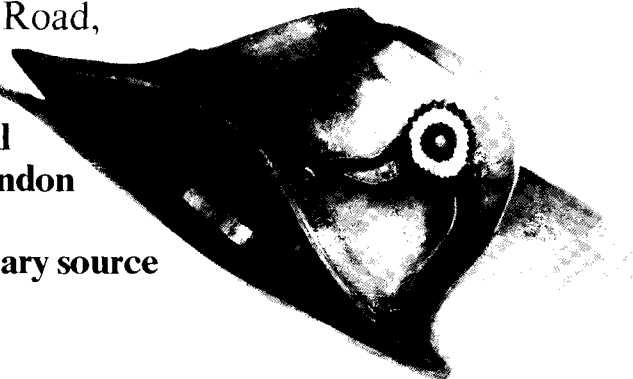
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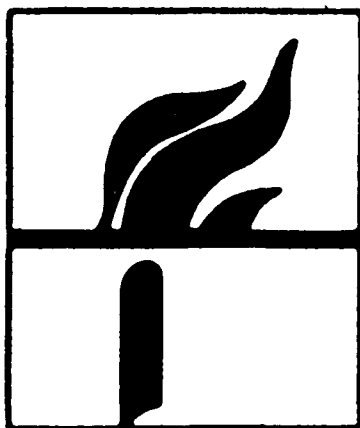
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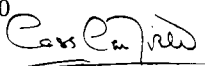
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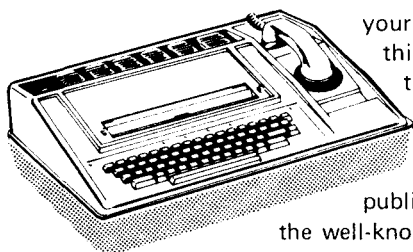
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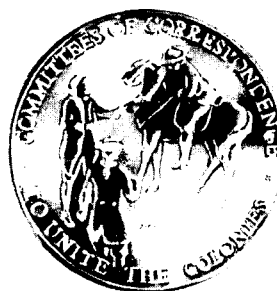
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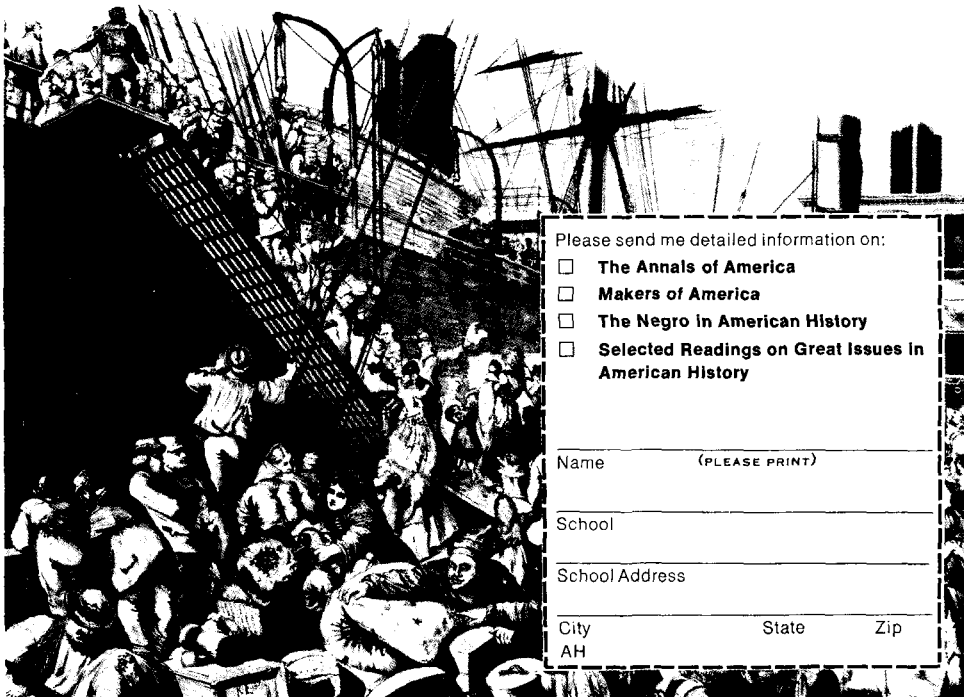
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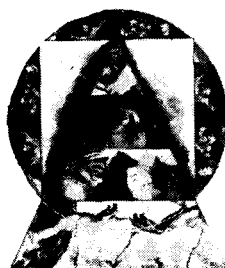


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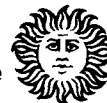
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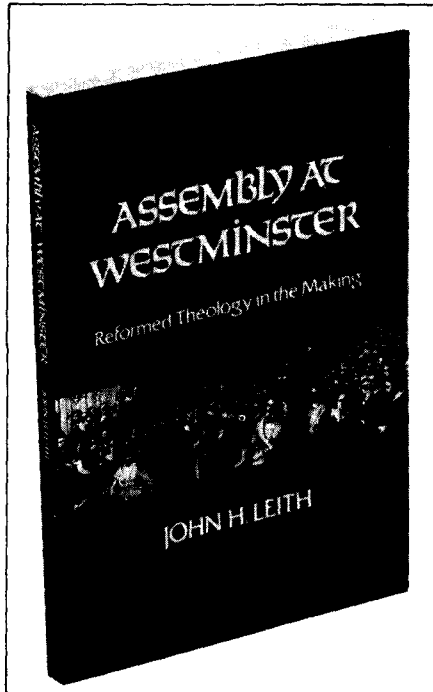
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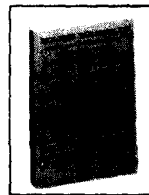
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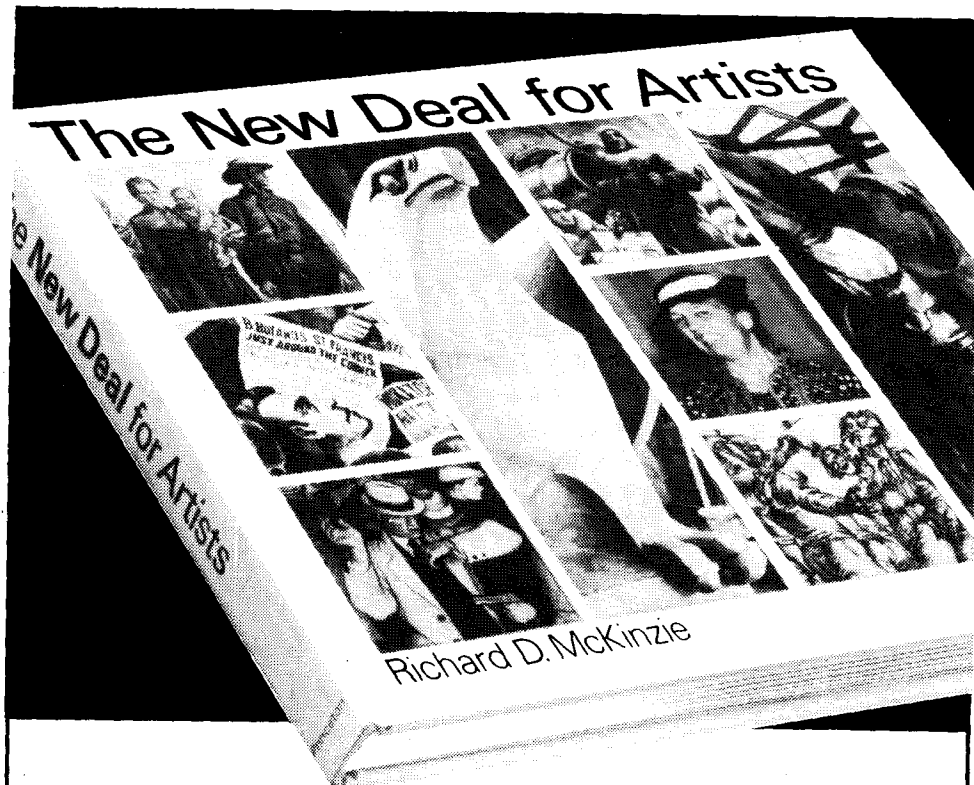
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